HIGH ENERGY PHYSICS FACILITIES

HEARING

BEFORE THE

SUBCOMMITTEE ON
ENERGY DEVELOPMENT AND APPLICATIONS

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

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HIGH ENERGY PHYSICS FACILITIES

WEDNESDAY, OCTOBER 19, 1983

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Development and Applications,
Washington, D.C.

The subcommittee met, pursuant to call, at 9:30 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Mr. Fuqua. The meeting will be in order.

We want to welcome everyone here today as we consider the Department of Energy's high energy physics facilities. This past July, the Department of Energy High Energy Physics Advisory Panel, the HEPAP group, as it's known, endorsed a report of its Subpanel on New Facilities for the U.S. high energy physics program. The subpanel's recommendations, if implemented by DOE, would have far-reaching consequences for the current program.

The subpanel recommended that the colliding beam accelerator under construction at Brookhaven National Laboratory be canceled.

Also, the subpanel called for the immediate initiation of a multi-trillion-electron-volt [TeV] proton-proton collider project, the superconducting super collider, known as the SSC, with the goal of physics experiments at this facility at the earliest possible date.

We will be hearing about these findings and recommendations in more detail from the chairman of the HEPAP Subpanel, Dr. Stanley Wojcicki, and the chairman of the HEPAP, Dr. Jack Sandweiss.

The HEPAP endorsement of the recommendation to cancel the CBA presents a reversal of the high energy physics community's past support for the project. This support was reaffirmed as recently as January 1982 by the HEPAP Subpanel on Long-Range Planning, chaired by Dr. George H. Trilling of the University of California at Berkeley.

The CBA project has also received support from this committee and Congress in the past. Through fiscal year 1983, about $200 million had been appropriated for the project, including some $120 million in construction funding. The fiscal year 1984 Energy and Water Development Appropriation Act, Public Law 98-50, included $28 million for the project; $23 million in operating expenses and $5 million in construction.

We expect to hear the DOE's plans for the disposition of these funds if the Department follows the HEPAP's advice and cancels the CBA, as well as its plans to protect the Government investment.

It's my understanding that Mr. Winn has a statement to make and without objection, we'll make that part of the record at this time, and also, without objection, photographs and television and recordings may be made during the course of the hearing.

[The opening statements of Messrs. Winn and Sensenbrenner follow:]

OPENING STATEMENT BY THE HON. LARRY WINN, JR.

Mr. Chairman, I welcome this opportunity to make a brief statement on basic research generally, and on the high energy physics program specifically.

The basic research program in the United States is certainly an impressive effort. Our total national R&D expenditures by all sectors in the United States exceed that of France, West Germany, and Japan combined. Besides the Soviet Union there are more R&D scientists and engineers active in the United States than any other country in the world. The investments in research and development and technological innovation are tremendous and have provided positive long-term effects on productivity and economic growth. On the other hand, we have seen some trends that are troubling. For instance, domestic patenting by U.S. inventors declined in the past decade while patenting in the United States by foreign inventors increased. We are told that several foreign countries stress science and mathematics literacy in their secondary schools to a greater degree than we do in the United States. Overriding all these positives and negatives is the necessity for development of knowledge, skills, and technology necessary to face increasingly complex national and international problems.

Over the many years that the Science and Technology Committee has provided strong support to basic research in the Federal government, we have seen impressive strides. We can claim significant increases in manufacturing productivity emenating from research and development programs. Our knowledge of the world in which we live has become more and more complete. The United States enjoys a favorable balance of trade in R&D-intensive manufactured products with international trading partners. The international science community has acclaimed the efforts of our fundamental research scientists through the award of many Nobel and other international prizes.

In all of basic reserach the high energy physics program is the one that seeks to understand the laws of nature which govern the fundamental behavior of matter and energy. We are here to consider that specific program today. This most basic scientific endeavor is managed by the U.S. Department of Energy as a national trust, with the goal of maintaining a comprehensive and productive national program and a world leadership position for the United States. As scientists have probed into the depths of matter, it has become necessary to use more intense beams of very energetic particles which are only available from high energy particle accelerators. The Science and Technology Committee has been continually asked to provide more powerful, more expensive facilities for these high energy physics studies.

Certainly many of my colleagues join me in the administration of our nation's scientific accomplishments. We support R&D and a strong science and technology base as a means to an end, not and end it itself. We remind the persons involved in the high energy physics program that federal support for research must meet three criteria—excellence, pertinence, and vision. I do not question the requests that come forward from the program except to remind those with the request that all budgets are limited and competition is keen. Congress must be convinced that these requests meet the criteria I have just mentioned.

Mr. Chairman, I join you in a warm welcome to Dr. Trivelpiece and to members of the High Energy Physics science community. We look forward to their testimony and will certainly find it beneficial.

OPENING STATEMENT OF HON. F. SENSENBRENNER, JR.

Thank you, Mr. Chairman, for this opportunity to extend a welcome to our distinguished witnesses.

I look forward to their testimony on the recommendations of the High Energy Physics Advisory Panel's subpanel on new facilities, contained in its report issued on July 20, 1983, and its impact upon the policies and programs of the Department of Energy.

The scientific peer review process, of which this report is a product of, has been used to prevent the intrusion of criteria, other than scientific, in the establishment of our scientific research priorities. While this isolation has served this country well for over three decades, I am concerned that the continued viability of this process is threatened unless other criteria are considered, on an equal basis, in this process.

The report of the subpanel on new facilities of the High Energy Physics Advisory Panel, and the subject of this morning's hearing, recommends the termination of construction of the colliding beam accelerator at Brookhaven National Laboratory and the immediate design and construction of a new proton-proton colliding beam facility, whose diameter is estimated in the report to be about 30 kilometers and which could cost the taxpayers as much as $8 billion when completed in the mid-1990's.

What is of concern to myself, and I'm sure many of my colleagues, is the 180 degree reversal of policy recommended in the report, from a 1982 HEPAP panel on this subject, which has the effect of negating the taxpayers' prior investment in the CBA facility.

Economics must be a criteria in the establishment of our scientific research facilities. This must be appreciated by the science community, and the department, if this country is to retain its preeminent position.

I look forward to our witnesses thoughts on this subject.

Thank you, Mr. Chairman.

Mr. **Fuqua**. At this time, we will hear from Dr. Trivelpiece, who is the Director of the Office of Energy Research at the U.S. Department of Energy. Dr. Trivelpiece, we're very pleased to have you here once again and we'll be pleased to hear from you at this time.

STATEMENT OF DR. ALVIN W. TRIVELPIECE, DIRECTOR, OFFICE OF ENERGY RESEARCH, U.S. DEPARTMENT OF ENERGY

Dr. **Trivelpiece**. Thank you, Mr. Chairman. I am pleased to be here this morning.

The place I want to start is perhaps a little background and some editorial comments. Human curiosity, really, doesn't seem to know any particular bounds. It's, I think, one of our most enduring and pervasive characteristics as humans.

We always seem to seek out the frontier, wherever it might lie, and that means whether or not it's the next mountain range or the next continent or, as it's turned out in the recent past, even the Moon. The English language has a convenient kind of an interesting way of helping us define where that frontier is. It turns out that, if you notice, you can take almost any word in the English language that applies to doing something and add "est" to it and you essentially have defined where the frontier is: the farthest, the deepest, the densest, the oldest, the shortest, the newest, and the smallest.

Well, we've progressed in our understanding of nature in rather interesting stages. We've gone through the Earth, air, fire, and water stage; we now call those solids, gases, plasmas, and liquids, and we know that they're made up of—those states of matter are made up of atoms.

At one point, atoms were the smallest, again the "est" coming in. Our curiosity has driven us to ask what atoms are made of and we know that atoms are made of electrons and protons and neutrons and for quite a few years, these were the smallest items.

There's no real clue yet to reveal that perhaps electrons aren't the smallest, but we know the protons and the neutrons have some strange things skulking about inside them and research has revealed that these things are called quarks and gluons. These are fanciful names, of course, but they are the terms which are used to define what are now perhaps the smallest bits of matter.

This field of high energy and elementary particle physics has been populated over the years by a collection of explorers and this collection has had mainly the one interest: Namely, what are the fundamental constituents of matter and what are the forces that influence their behavior?

Like most explorers, the high energy physicists need tools. They don't need ships or rockets or airplanes, although I must confess a lot of them seem to spend a rather inordinate amount of time wandering around in airplanes, but be that as it may, what they need are accelerators and detectors and the kind of ancillary scientific apparatus that goes with this.

By some standards, this apparatus seems to be rather expensive, but if you really look back over the years, you find that there's a remarkable number of benefits that have accrued to society from the results of the fundamental discoveries that have been made in the science of this area.

I've described these facilities and the benefits derived at previous hearings. I'm not going to repeat those here, but what I want to point out is that this has been basically a good investment on the part of the United States and that the costs have been more than justified and been repaid by the commerce and trade and other activities that have flowed directly out of this kind of research.

Well, the curiosity-driven character of this field and the human characteristic of competitiveness have led to some difficult situations in the past in terms of the level of funding that's available and how it's to be directed and who is to use it and so on. This hasn't always been a harmonious working relationship between the scientists in the field and, in some cases, collectively it hasn't been too harmonious between them and the agency that has supported them.

But in spite of the discord that has taken place in the past, and some of it which may come up in the course of the discussions today, it has been remarkable in that when all the shouting is sort of done, there's generally a strong agreement among those that are practitioners in this field as to what the next most important step is, what are the tools that are needed in order to accomplish that step, and how to go about doing that.

And in the real world of constraints in which they and we all have to live, they have shown a remarkable characteristic in the past of having terminated a number of facilities in order to have the resources to continue making progress at the frontier, whatever the smallest or newest thing is to be explored, and with your permission, I would like to have included in the record a listing of those facilities that have been terminated in the past and how that has gone.

Mr. FUQUA. Yes, we'll make that part of the record.

[The information follows:]

High Energy Physics

Accelerator Facilities Shutdown

Facility	Energy (GeV)	Particle	First Beam	Shutdown Date
Fixed Target Facilities				
Stanford Linac (HEPL)	1.2	Electron	1950	1969
Cal. Tech Synchrotron	1.5	"	1952	1970
Cornell Synchrotron	1.3/2.2	"	1955	1968
Cambridge Electron (CEA)	6.3	"	1962	1970*
Cornell Synchrotron	12.0	"	1967	1977**
Cosmotron	3.0	Proton	1952	1966
Bevatron	6.2	"	1954	1974***
Princeton-Penn. (PPA)	3.0	"	1963	1971
Zero Gradient Synchrotron (ZGS)	12.7	"	1963	1979
Fermilab Synchrotron	500.0	"	1972	1982****
Colliding Beam Facilities				
Cambridge Storage Ring	3.0x3.0	Electron/ Positron	1970	1973

* Converted for colliding beam use.
** Now serves as injector for CESR colliding beam facility
*** Subsequently converted to Bevalac Heavy Ion Machine
**** Now serves as injector for the Tevatron

Dr. **Trivelpiece**. Well, I'm bringing this up in this context here today because Dr. Sandweiss, who is the chairman of the High Energy Physics Panel, has addressed some rather serious questions. I asked him to look into this last year and to tell us in the Department what it was that they thought was the most important thing to do in the field, and Dr. Sandweiss and Dr. Wojcicki, as you indicated, are going to discuss their conclusions, and I'm going to just briefly summarize what the Department's plans are with respect to those recommendations and let them talk about the physics and the nature of the recommendations themselves.

As you indicated, one of their main recommendations is that we begin the steps to plan for a multi-TeV proton collider. This new accelerator is called the SSC, for superconducting super collider, and it's needed to create particles in this so-called TeV mass range, and the reasons why this is important are going to be discussed by my two colleagues here.

The other recommendation that I would like to discuss briefly is the fact that the Department is going to request termination of the colliding beam accelerator project, based on the HEPAP recommendations. Yesterday, you received a letter from Secretary Hodel informing you of the Department's plans not to proceed with that project and in so doing, to terminate the Isabelle effort.

[See appendix II, p. 161 for letter mentioned above.]

These two recommendations are coupled in some sense because what we are planning to do with the funds that would be redirected out of the CBA magnet research is to do the preliminary research for the superconducting super collider.

There have been many guesses as to what such a facility might cost and I'm not going to engage in such a guessing activity. I think it is extremely premature to do so and, in fact, rather counterproductive.

Rather, I think what is essential is to begin a very careful process of the preliminary efforts to define and scope out what such a project would cost so that at such time as it can be accurately estimated, we could make an appropriate proposal and bring it forward to the Congress with accurate estimates in it.

These are based in part on, of course, what HEPAP has recommended in terms of what we should do with the transition from 1983 to 1984. In addition to this panel, there is another subpanel that I asked Dr. Sandweiss to set up and that was one that has been chaired by Dr. Panofsky of Stanford to make some recommendations to HEPAP as to what the transition activity should be in order to lay out a proper plan to define what a superconducting super collider should be.

The nature of the distribution of the 1984 funds that we're seeking redirection for were not included in my formal testimony which I did submit for the record, and I would like to have the additional data sheets that were omitted included as part of the record.

Mr. **Fuqua**. Yes; we will make that part of the record.

Dr. **Trivelpiece**. Thank you.

[The information follows:]

Disposition of $23M of FY 1984 Funds
Identified for CBA R&D
(B/A in Millions)

	Pres. Budget FY 1984	Revised Plan FY 1984
	$23.0	$23.0
CBA R&D	23.0	-
CBA Close Out	-	4.3
AGS R&D Increase	-	0.7
Advanced Accelerator R&D at BNL	-	10.1
Advenced Accelerator R&D at Other Locations	-	7.9
Fermilab	-	4.0
LBL	-	2.0
Unallocated	-	1.9

Analysis of
BNL Operating Funds in FY 1984
(B/A in Millions)

	Pres. Budget FY 1984	Plan * Based on Approp. FY 1984	Revised** Plan FY 1984
Total HEP Operating Funds	$349.5	$342.5	$342.5
Total BNL Operating Funds	68.6	66.6	61.2**
High Energy Technology	28.0	28.0	20.1
CBA R&D	23.0	23.0	-
AGS R&D	5.0	5.0	5.7
CBA Close Out	-	-	4.3
Advanced Accelerator R&D	-	-	10.1
AGS Operations	33.6	31.9	34.2
Physics Research	7.0	6.7	6.9

* This plan takes into account the Congressional reduction in the overall HEP Physics Research and Facility Operations subprograms and ignores the redirection of CBA R&D.

** Revised plan reflects actions relative to BNL based on HEPAP's recommendations:

- high priority for AGS operations
- high priority for SSC
- Do not approve the CBA

Advanced Accelerator R&D in FY 1984 Preliminary to a Future High Energy Physics Facility

(B/A in Millions)

BNL	$ 3.0	Development of high field Nb_3Sn superconducting cable
	5.5	2-in-1 magnetically linked superconducting magnets (in collaboration with LBL)
	1.1	Accelerator design, studies of generic advanced accelerator site issues
	0.5	Cryogenics and other technical systems
	\$10.1	
LBL	\$ 2.0	5-6 Tesla NbTi 2-in-1 magnetically linked superconducting magnets with early industrial participation in design and tooling (in collaboration with BNL)
Fermilab	\$ 4.0	5-6 Tesla NbTi 2-in-1 magnetically unlinked superconducting magnets
Unallocated	\$ 1.0	Design, trade-off, optimization studies, and accelerator physics
	0.5	Superferric magnet design and development
	0.4	Cryogenics and other technical systems
Total FY 1984 Adv. Accel. R&D	\$18.0	

Dr. **Trivelpiece**. We're, of course, very sensitive to the impact that the actions that we have taken will have on Brookhaven National Laboratory, on the personnel and the research and so on, but Brookhaven would have, and will have, we hope, a major role in the superconducting super collider effort.

It's really not the main purpose of the hearing to go into this, but I'm going to identify something that is in my written testimony; namely, that we are also requesting the reprogramming of $5 million of the Isabelle construction funds into an activity that would connect the alternating gradient synchrotron with the tandem Van de Graaff accelerator at Brookhaven.

This would establish a heavy ion research capability at Brookhaven which, if it proved successful, would be the preliminary step toward eventually connecting the ISABELLE tunnel with the alternating gradient synchrotron to establish a relativistic heavy ion capability there at Brookhaven, so this might provide something that could be done with the tunnel at some point in the future.

Before turning to the other members in the panel here, I would like to make one comment concerning the Federal Advisory Committee Act, which I think is important. The Department, under the Federal Advisory Committee Act, receives advice from a number of committees. These committees are chartered in accordance with the law.

One aspect of that is that we receive advice only from those committees and not from other groups that have not been chartered, but in the charter provision, it is possible for the committee—the panel, in this case the High Energy Physics Advisory Panel—to have subpanels that report to it and in that sense, the subpanel advice does not come directly to the Department, but it is a tool that the HEPAP uses in order to develop its case and make recommendations to the Department.

So Dr. Wojcicki is an adviser to Dr. Sandweiss and in that sense, not an adviser to the Department. So legally we can take note only of the information provided to us directly from the High Energy Physics Advisory Panel and not directly from the subpanel.

Mr. Chairman, that concludes my opening remarks and I'd be happy to answer questions now or I presume you were planning on having the other gentlemen speak first.

[The prepared statement of Dr. Trivelpiece follows:]

Statement of Alvin W. Trivelpiece
Director
Office of Energy Research
Department of Energy
Subcommittee on Energy Development and Applications
of the
House Science and Technology Committee
October 19, 1983

I am pleased to appear before you today to discuss recent developments in regard to U.S. high energy physics facility plans. I will briefly review the findings and recommendations of the Department's High Energy Physics Advisory Panel (HEPAP). I will discuss two of their recommendations in detail and then tell you about DOE's plans with regard to these recommendations.

Late in 1982 I requested that Dr. Sandweiss, Chairman of HEPAP, review the facility needs of the U.S. High Energy Physics Program. A copy of the letter confirming my request is attached to my testimony. He appointed a Subpanel on New Facilities which first met in February 1983. It was asked to consider and make recommendations relative to the scientific requirements and opportunities for a forefront U.S. High Energy Physics Program in the next five to ten years, to make specific recommendations with respect to possible new construction items for FY 1985, and to include a definite recommendation concerning the proposed Colliding Beam Accelerator (CBA) at Brookhaven National Laboratory (BNL).

After the initial organizational meeting in Washington, DC, the Subpanel held three-day on-site meetings at the Fermi National Accelerator Laboratory (Fermilab), BNL, and Stanford Linear Accelerator Center (SLAC), which also included interaction with university users and other laboratories, and had major deliberative meetings at the National Academy of Sciences Study Center in Woods Hole, Massachusetts and at the Nevis Laboratory, Columbia University.

The Subpanel report was reviewed and deliberated on by HEPAP at its July 11-12 meeting. HEPAP unanimously endorsed the recommendations of the Subpanel report and forwarded the Subpanel report to me as its own recommendation on July 12, 1983.

Their recommendations are:

1. The immediate initiation of a multi-TeV high-luminosity proton-proton collider project with the goal of physics experiments at this facility at the earliest possible date.
2. The rapid completion of current construction projects, Tevatron and the SLAC Linear Collider (SLC), the upgrading of the Cornell Electron Storage Ring (CESR) and associated detectors, and the thorough utilization of all existing facilities.
3. That Fermilab not proceed at this time with the Dedicated Collider.
4. That the Colliding Beam Accelerator (CBA) project at Brookhaven not be approved.
5. That technology research and development, particularly advanced accelerator research and development, be strongly supported.

I will discuss in detail the first recommendation pertaining to initiation of a multi-TeV high-luminosity proton-proton collider called the Superconducting Super Collider (SSC) and the fourth recommendation pertaining to not approving the proposed CBA project at BNL.

HEPAP recommends the immediate initiation of an effort that would lead to the SSC, which is envisioned as a proton-proton collider with an energy of 10-20 TeV per beam and a high luminosity (i.e., high particle interaction rate). This recommendation is given their highest priority and they recommended that the project preparation begin in FY 1984 in order to meet the goal of completion in the first half of the 1990's. They recommend that the project should begin with an intensive R&D phase over three to four years to establish a cost-optimized design for the facility. Such a facility would have immense physics potential to advance continued understanding of the fundamental nature of matter.

The recognition of the need for and importance of a multi-TeV proton-proton collider grew strong during the 1982 American Physical Society Division of Particles and Fields Summer Study on Elementary Particle Physics and Future Facilities, June 28 - July 16, 1982. This recognition is rooted in recent important accomplishments in experimental and theoretical high energy physics research which have provided the new insights that strongly suggest that exploration of the energy region where particles with fundamentally new properties and a mass of a few TeV can be produced is the next major step required to advance our knowledge of fundamental processes. This exploration can be accomplished with a high-luminosity proton-proton collider with an energy of 10-20 TeV per beam. Such a device now lies within reach of present day technology and has been made feasible by the recent major successes resulting from the U.S. investment in superconducting magnet technology.

During this decade, existing and planned particle accelerators will permit the study of interactions between elementary constituents (quarks, gluons, and leptons) at energies up to a few tenths of a TeV. This energy is not sufficient to address the now open and fundamental question of how the masses of constituents themselves are generated in nature. This question is not answered satisfactorily in current theories and clearly requires experimental input to guide progress in understanding. All known approaches to answering this question predict specific new phenomena, especially new forms of matter with unique properties, in the mass range of a few TeV. The SSC would provide the capability to discover and study such new forms of matter and, consequently, provide significant input to our understanding of the fundamental properties of nature. This project is the essential next step in the U.S. High Energy Physics Program toward this understanding and also allows us to reap the benefits that follow from the substantial investment we have made in superconducting technology.

In view of the scientific priority of this project, the sense of interest and urgency in the community, and the long time scale for R&D and construction, the Department proposes to initiate advanced accelerator R&D in order to consider proceeding with the construction of a high energy physics facility in the 20 TeV per beam energy range.

Now I will turn to the Colliding Beam Accelerator (CBA) recommendation. This project was proposed as the successor to the ISABELLE project. A major focus of the initial physics justification for ISABELLE was to test the theory that two of the fundamental forces in nature, the electromagnetic and weak interactions are, in fact, different manifestations of a single force and to experimentally explore the precise structure of the combined (electroweak) interaction. The energy and high interaction rate of the ISABELLE project were considered well matched to these physics questions. Prior to the 1983 HEPAP Subpanel on New Facilities for the U.S. High Energy Physics Program, the ISABELLE project was considered by HEPAP in 1974, 1975, 1977, 1980, and 1981. In 1974, HEPAP endorsed ISABELLE as one prong of a three-pronged attack on the frontiers of high energy physics and recommended increased R&D effort. In 1975, HEPAP endorsed the previous recommendation and recommended that authorization be considered in FY 1977. In 1977, HEPAP gave top priority to the construction of ISABELLE at an increased energy of 400 GeV per beam compared to the earlier plans for 200 GeV per beam. Construction was initially, partially authorized in FY 1978. In 1980 and 1981 HEPAP recommended rapid completion of ISABELLE, although there was growing recognition and concern over the technical and management difficulties which were becoming evident. The 1981 HEPAP recommendation was qualified to reflect this concern.

As a result of both technical and management difficulties with the ISABELLE project, the project cost and time schedules increased substantially beyond the original plan. In view of this situation, I did not request construction funds and Brookhaven was instructed in FY 1982 to revert to an R&D mode in which they were to concentrate on solving the technical and management problems and on examination of possible alternative facilities that were scientifically important and could be built at a lower cost using the existing tunnel. In April 1983, BNL submitted a proposal for the CBA, which in scope and performance was virtually identical to the ISABELLE project. The Department carefully reviewed the CBA proposal and sought the advice of HEPAP on whether proceeding with the CBA was the proper course of action for the U.S. High Energy Physics Program from a scientific point of view. The Department has received a recommendation from HEPAP that the CBA project not be approved.

In coming to the recommendation that the CBA facility not be built, HEPAP noted that even though there is significant probability of important new physics discoveries at CBA, the existing CERN SPS proton-antiproton collider and the Tevatron I collider, which is under construction at Fermilab, will explore much of the physics that would have been accessible to the CBA. HEPAP concluded, based on recent major physics accomplishments and insights, that significantly higher priority now should be given to the study of the interactions of particles at energies far above the capability of the CBA, in an energy region where there are no existing or planned facilities in the world.

HEPAP believes that there is an urgent need for a forefront facility that would assure a world-leading U.S. High Energy Physics Program from the mid-1990's into the next century, and that proceeding with CBA would interfere with this goal. HEPAP advised that available financial and manpower resources could best be utilized for a higher energy facility which would have the potential for making important discoveries in a domain in which there are no existing or planned facilities in the world. This scientific emphasis on the importance of having a facility capable of providing the highest attainable energy constitutes the basis of the HEPAP recommendation not to approve CBA.

After carefully reviewing the recommendations from HEPAP, the Department has decided not to approve the CBA and to terminate the ISABELLE project.

I will now describe the Department's plan for redirection of the $23 million of FY 1984 operating funds identified in the President's budget for CBA R&D, for disposition of the remaining ISABELLE construction funds from FY 1983 and prior years, and for redirection of the $5 million of FY 1984 funds appropriated for ISABELLE construction.

The FY 1984 funding for BNL accelerator R&D, which had been planned to focus on R&D related to the CBA project, will be redirected to increased R&D related to Alternating Gradient Synchrotron (AGS) improvements, costs for close out of those R&D activities of specific value only to CBA, and advanced accelerator R&D at BNL and elsewhere for a future high energy physics facility. Related to the redirection of CBA R&D efforts, there will be a net reduction of $7.9 million in the High Energy Technology portion of the BNL operating budget for FY 1984.

Consistent with the recommendations by HEPAP regarding the high priority of the AGS and for the thorough utilization of existing facilities, we are providing some additional funding to improve the operating level of the AGS over that earlier planned.

With regard to the termination of the ISABELLE construction project and the CBA R&D efforts, the Department plans to work with BNL to determine the amount of funding remaining, to evaluate outstanding procurements to determine if they should be continued or terminated in whole or in part, and to develop a plan for rapid and orderly close out. The Department is concerned about protecting the Government's investment in already-built facilities at BNL and assuring that facilities with known future uses are brought to an appropriate state of completion. In particular, I believe that the cryogenic refrigeration system and the Service Building will be important to support the planned substantial role for BNL in the R&D for a future high energy physics facility. I expect that these activities can be completed with the remaining construction funds (approximately $3 million) from FY 1983 and prior years' appropriations.

The Department is considering a plan to establish a new heavy ion research capability at BNL. BNL has proposed a project to provide a link between the existing Tandem Van de Graaff accelerator and their AGS facility which will be modified to accommodate acceleration of heavy ions. Establishing this capability would require redirection of the $5 million appropriated for ISABELLE construction in FY 1984 to the AGS improvement program. Eventually this heavy ion research complex could be combined with the existing ISABELLE tunnel to provide a relativistic heavy ion collider capability for nuclear physics research.

Next, I would like to discuss a number of steps the Department has taken with regard to the recommendation relative to the SSC. The SSC is envisioned as a project that will proceed in two phases, Phase-I (R&D) and Phase-II (Construction). At present, we are in what might be called Phase-Zero in which we are focusing on those actions that must be accomplished before Phase I could begin. As a follow-on to these preliminary activities, approval would be sought for the Phase I R&D effort.

A major thrust of present activities is the FY 1984 program of advanced accelerator R&D preliminary to a future high energy physics facility. I requested that HEPAP form a Subpanel to provide advice on the advanced accelerator R&D program preliminary to a future high energy physics facility. The Subpanel, which is chaired by Dr. Panofsky, Director of the Stanford Linear Accelerator Center, held its first meeting on September 7-9, 1983. An informal task group of leading U.S. accelerator experts, led by Dr. Tigner, Cornell University, had earlier developed a preliminary Phase I R&D plan. The Panofsky Subpanel reviewed and modified this four-year plan. It also heard presentations from DOE laboratories and other interested parties and arrived at a consensus on conclusions and recommendations on R&D activities needed for FY 1984 consistent with the long-range R&D plan. The Department has developed a plan for redirection of FY 1984 resources and manpower toward advanced R&D preliminary to a future high energy physics facility. The $18 million for this R&D effort would be obtained from the $23 million previously planned for CBA R&D.

There are two features I would like to call to your attention. First, the Department's plan assigns a major role in the advanced accelerator R&D program to BNL. Second, the early R&D program focuses heavily on superconducting magnets because the choice of magnet design impacts critically on the size and cost of the facility and on site considerations.

The Panofsky Subpanel also proposed a number of workshops during the next year to study and determine tradeoffs of various high energy physics accelerator design issues. These activities will provide important backup knowledge against which to judge potential future requests for funding of Phase I of the SSC.

Finally, there is the question of management for Phase I activities. A key recommendation of HEPAP was that an interim line manager should be selected as soon as possible.

A lot has happened in the short period since receipt of the HEPAP recommendations. I have presented to you today the status of those decisions which the Department has made and of plans that the Department has developed and plans to implement in the near future.

I am optimistic about the future of high energy physics in the U.S. The proposed SSC is a bold step which is necessary to remain at the forefront of physics for the remainder of this century and into the next.

Mr. Chairman, that concludes my testimony. I would be pleased to respond to any questions you might have.

Department of Energy
Washington, D.C. 20585

January 27, 1983

Professor Jack Sandweiss
Chairman, HEPAP
Department of Physics
Yale University
New Haven, CT 06520

Dear Jack,

The collection of people that have contributed to our improved understanding of the fundamental constituents of matter and the forces that influence their behavior have done a remarkable job. Over the years this group has unraveled one seemingly unfathomable mystery after another. At each stage of this process there has been a need to have some combination of tools, talent, financial resources, luck, and faith. In some cases, the "combination" has involved gifted insights by a single, or a few, individuals and in other cases, it has involved large teams, massive apparatus, and years of hard work.

As with most such endeavors that seek fundamental knowledge and are tax-supported, there is rather intense competition for limited resources. The evolution of high energy physics from many small research projects at universities to a few large-facility major research centers has not been without competition, debate, and the need to make difficult choices from among several almost equally desirable possibilities.

A certain amount of this competition is healthy, but it can also be destructive. I am concerned that the present debate on directions and distribution of resources within the high energy physics community is taking on some unhealthy characteristics that have the potential for making the situation worse for all concerned. I am only too well aware of the budgetary problems that this and other science programs are facing, and I am sympathetic with the frustrations that have led to the acrimony and quasi-lobbying efforts of some of the high energy physics community. However, these efforts are not helping to resolve the problems, and I would ask you, through HEPAP, to encourage a more restrained and statesmanlike approach by all members of the community.

The High Energy Physics Advisory Panel (HEPAP) provides an organized and open forum in which the community can debate important issues and reach a consensus on the best course of action to ensure that effective progress is made in the field. The Panel and its special subpanels are well respected in Washington for the way in which they have faced up to tough issues and provided sound advice. In that regard, I am requesting that you work together with Jim Leiss, the Associate Director for the Office of High Energy and Nuclear Physics, to develop a charge letter for the 1983 New Facilities Subpanel that makes it clear that what is needed at this time are unequivocal recommendations on the research programs that will, in their opinion, make the most effective progress. I then expect HEPAP to debate these recommendations and to endorse, qualify, or reject them, as might be appropriate.

Whatever the outcome of your deliberations, I hope that you will not deliver up "soft" recommendations at the end of this process. By that I mean that it will not be particularly useful to have recommendations that we could do thus and so if we just had so much funding. What are the most important high energy phyics facilities for the United States to have, regardless of where they might go or how much they might cost? What are the elements of a balanced high energy physics research program that will ensure continued progress in experiment, theory, and the associated advanced technology. These are the kinds of questions that the Subpanel and HEPAP will have to consider in making their recommendations.

I would like to discuss with you and Jim Leiss the Subpanel Charge which you develop, prior to its delivery.

Sincerely,

/s/

Alvin W. Trivelpiece
Director, Office of
Energy Research

Mr. Fuqua. Yes, I will. I just have one—one brief question to clarify. You mentioned the $5 million and in the letter from Secretary Hodel, it says a redirection, but it doesn't say "reprograming." Is that intended to be a reprogramming action?

Dr. Trivelpiece. I think redirection is the right term, but I'm not sure that—perhaps by——

Mr. Fuqua. We'll check that. What about the other $23 million?

Dr. Trivelpiece. The other $23 million, the distribution of that is in the data sheets which will be included in the——

Mr. Fuqua. OK, because that was not——

Dr. Trivelpiece [continuing]. Record. Approximately $4 million of that, we are proposing to use to, in effect, put the tunnel and the refrigeration system that is at Brookhaven in a safe state so that it can be used at a later stage, in a sense, mothballing the facility in a responsible manner.

The other part will be used to increase some of the activities on the alternating gradient synchrotron; some of it will be used for superconducting super collider research at Brookhaven; and a small portion of it will be redirected to some other laboratories for some work in superconducting magnets also relative to the SSC project.

Mr. Fuqua. Thank you.

Dr. Wojcicki.

STATEMENT OF DR. STANLEY G. WOJCICKI, CHAIRMAN, DEPARTMENT OF PHYSICS, STANFORD UNIVERSITY, AND CHAIRMAN, 1983 HEPAP SUBPANEL ON NEW FACILITIES

Dr. Wojcicki. Mr. Chairman, first of all, let me thank you for this opportunity to present the recommendations of the High Energy Facility Subpanel to you and the rest of the committee.

With your permission, I would like to use the overhead projector for the transparencies.

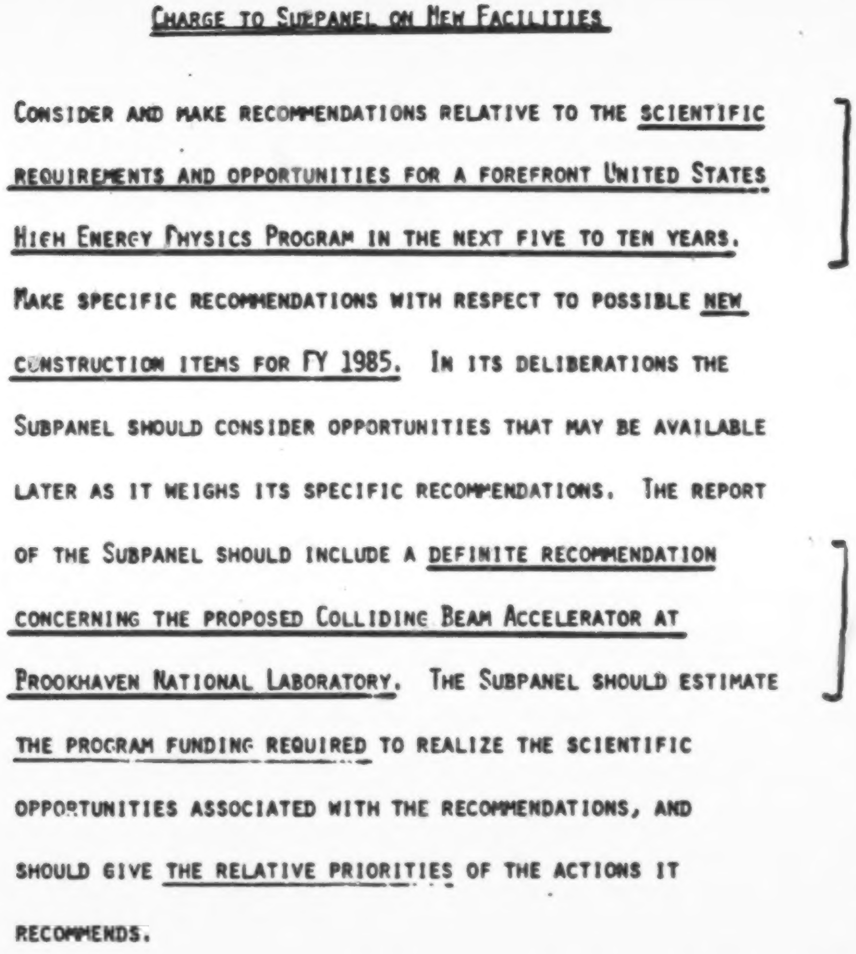
Mr. Fuqua. Certainly.

Dr. Wojcicki. I hope maybe my presentation will be clearer.

Mr. FUQUA. One moment, they need a hand mike for you.

Dr. WOJCICKI. OK, let me start out by just putting on the projector the charge to the subpanel.

[Slide 1 follows:]



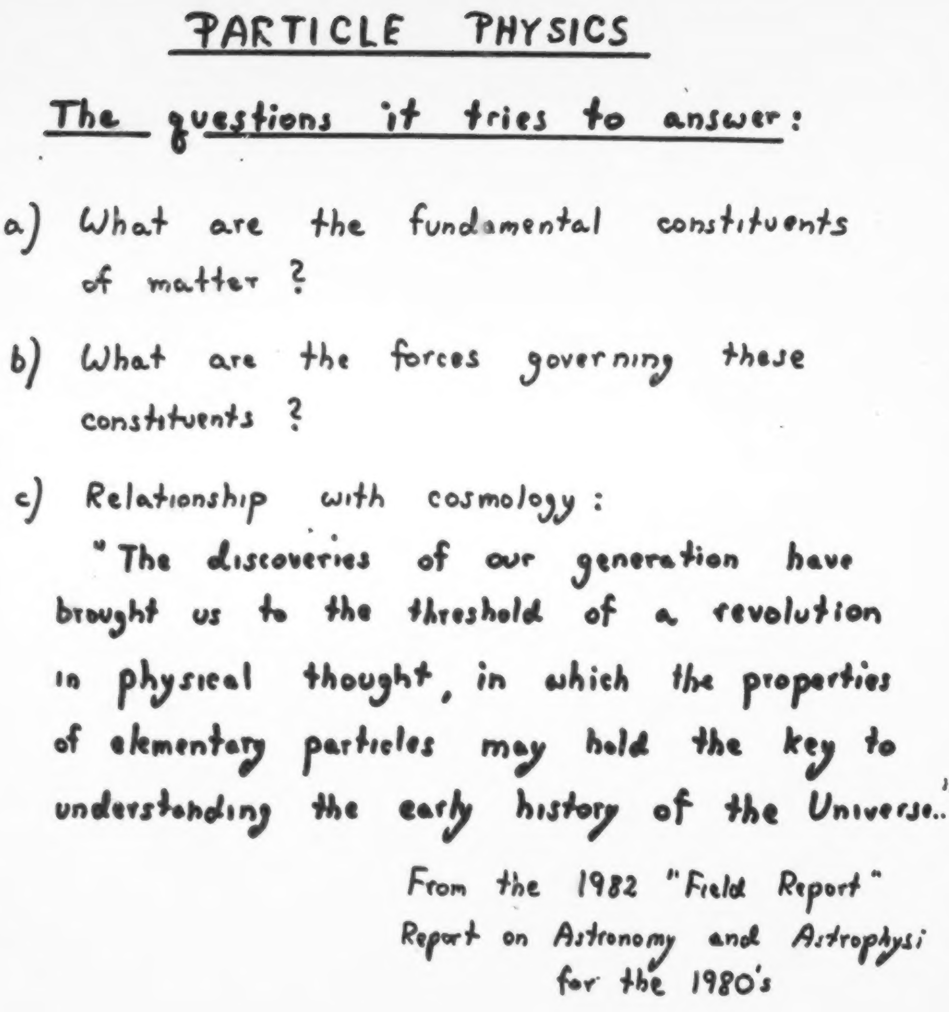
Charge to Subpanel on New Facilities

Consider and make recommendations relative to the scientific requirements and opportunities for a forefront United States High Energy Physics Program in the next five to ten years. Make specific recommendations with respect to possible new construction items for FY 1985. In its deliberations the Subpanel should consider opportunities that may be available later as it weighs its specific recommendations. The report of the Subpanel should include a definite recommendation concerning the proposed Colliding Beam Accelerator at Brookhaven National Laboratory. The Subpanel should estimate the program funding required to realize the scientific opportunities associated with the recommendations, and should give the relative priorities of the actions it recommends.

SLIDE 1

Dr. WOJCICKI. I shall not take the time to read the whole charge, because since the discussion of this committee concerns itself mainly with two of the recommendations of the subpanel, I would just like to point out the specific charge to the subpanel which addresses itself to those recommendations, namely the scientific requirements and opportunities for a forefront United States high energy physics program in the next 5 to 10 years. So that's the recommendation regarding the SSC and also a definite recommendation concerning the proposed colliding beam accelerator at Brookhaven National Laboratory.

Let me maybe start out this presentation by just reminding you about the questions that the particle physics addresses [slide 2].



PARTICLE PHYSICS

The questions it tries to answer:

a) What are the fundamental constituents of matter?

b) What are the forces governing these constituents?

c) Relationship with cosmology:

"The discoveries of our generation have brought us to the threshold of a revolution in physical thought, in which the properties of elementary particles may hold the key to understanding the early history of the Universe."

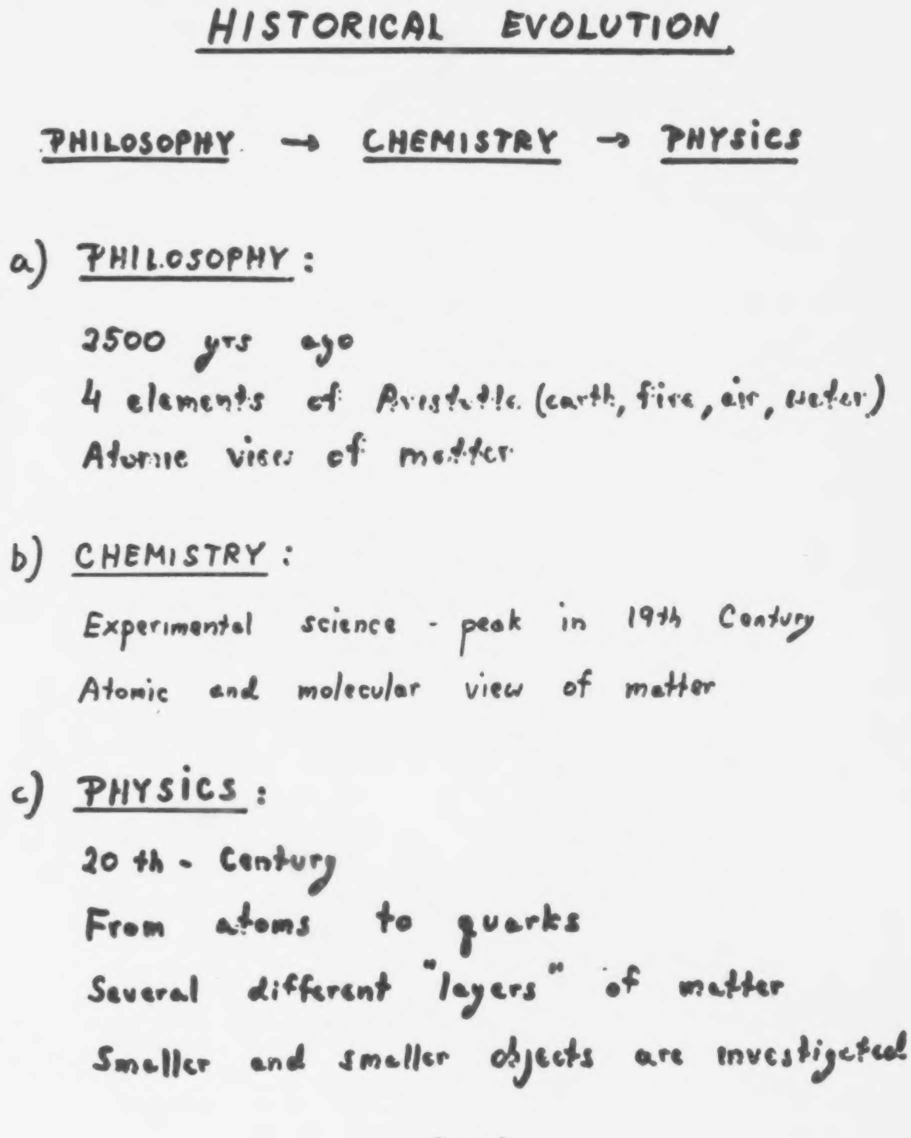
From the 1982 "Field Report"
Report on Astronomy and Astrophysi
for the 1980's

SLIDE 2

The two fundamental questions, I think always have been: What are the constituents, the basic constituents of matter; and second, what are the governing forces which are relevant as far as these constituents are concerned.

What has happened actually in the last 10 or 15 years has been a very exciting intellectual development, namely relating a lot of the particle physics with the questions of cosmology; namely, what is the origin of the universe? I think this is probably best expressed here by this quote from the Field Report that some of the questions that the particle physicists are addressing themselves to may really lead to the ultimate clue as to the origin of the universe. And as a matter of fact, I was very gratified to hear this morning that the Nobel Prize in physics this year was awarded to Professor Fowler of Cal Tech and Professor Chandrasekhar of Chicago for work having to do with the nuclear syntheses at the beginnings of the universe. So there are exactly the same kind of questions that I think I am mentioning here.

To give you a historical perspective of this evolution of these questions, we could say we went sort of from philosophy to chemistry to physics. Clearly these are age-old problems. Mankind has gazed at the stars and wondered what we are made of, you know, ever since time immemorial. I think the ancient Greeks sort of philosophied about some of these problems [slide 3].



HISTORICAL EVOLUTION

PHILOSOPHY → CHEMISTRY → PHYSICS

a) PHILOSOPHY:

2500 yrs ago

4 elements of Aristotle (earth, fire, air, water)

Atomic view of matter

b) CHEMISTRY:

Experimental science - peak in 19th Century

Atomic and molecular view of matter

c) PHYSICS:

20 th - Century

From atoms to quarks

Several different "layers" of matter

Smaller and smaller objects are investigated

SLIDE 3

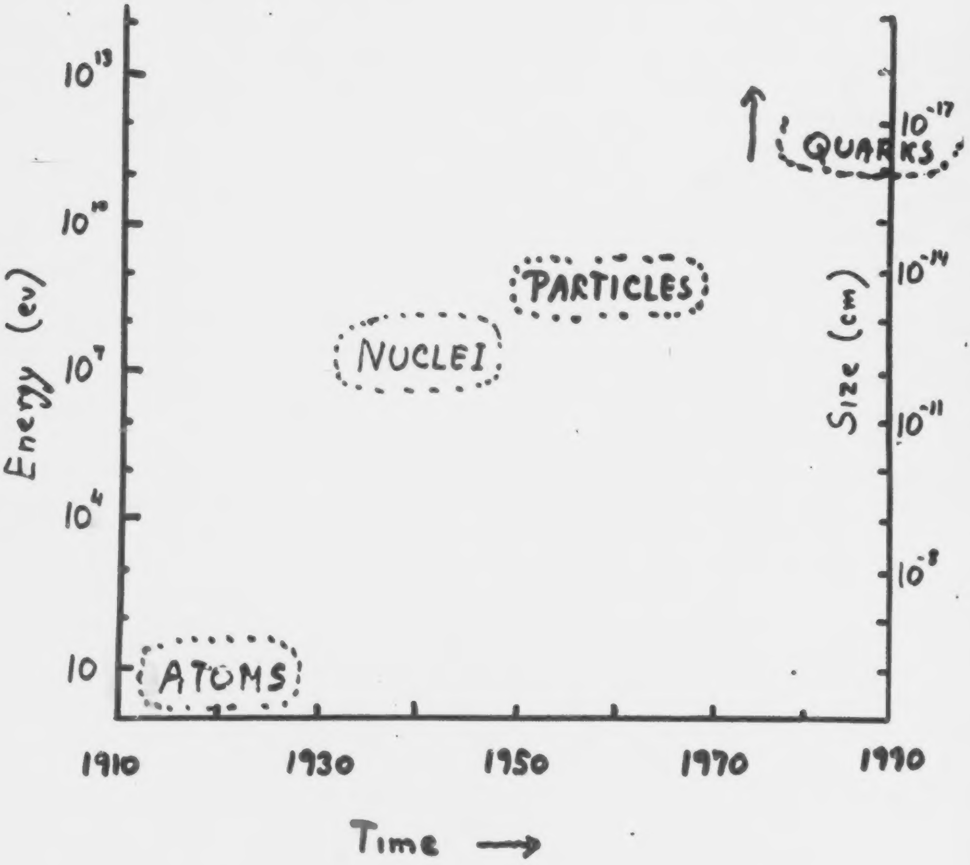
On the other hand, the experimental science only started in the last two or three centuries. I think we saw a foundation of the atomic and molecular chemistry relayed through the work of chemists in the 18th or 19th century, which led to the atomic and molecular point of view.

The physicist, you might say, got into the act at the beginning of this century and I think probably the best way to characterize what has happened in the last 80 or 100 years, basically smaller and smaller constituents were being examined.

I think an analogy that is frequently used is sort of peeling off skin—peeling an onion. You peel off one layer of the onion and you understand that, and then you see the next one. Very much, that's what I think has happened. We have gone from a relatively large scale like the atoms to what we believe are fundamental constituents today, namely the quarks.

Dr. WOJCICKI. I think—let me maybe indicate this in a little bit more detail in this transparency over here. What I've tried to indicate here is first of all the progression of time in this century, at the same time, smaller and smaller distances on a logarithmic scale in this direction and at the same time, higher and higher energy on this axis. And I think it is the fundamental postulate, basically, of quantum mechanics that to investigate smaller and smaller distances, you do need higher and higher energies. These two are intricately related together and therefore basically there is this one-to-one correspondence between these two scales [slide 4].

PROGRESS WITH TIME



Smaller distance ↔ higher energy

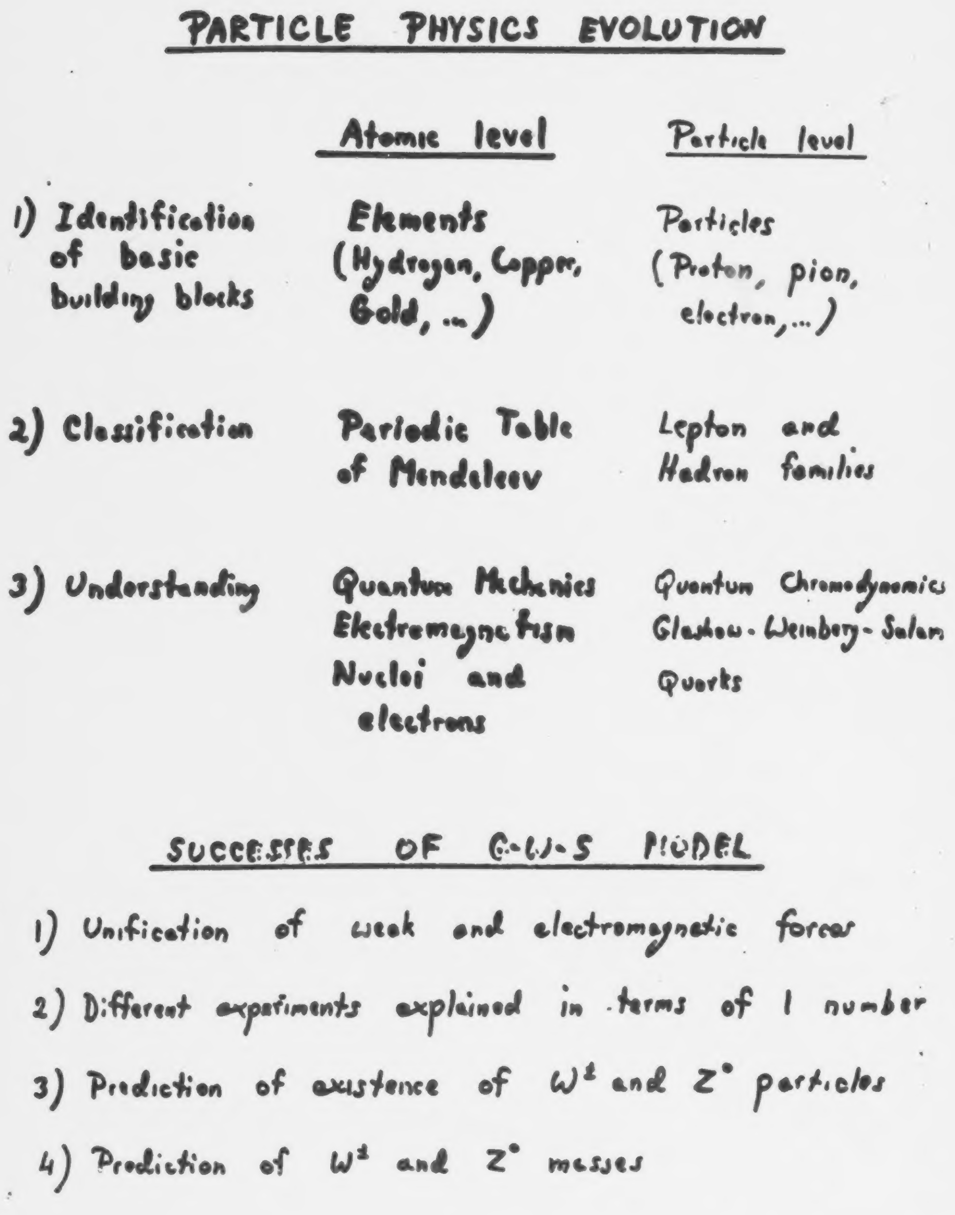
Slide 4

I think maybe the progress can be understood by looking at these successive layers or substructures of matters that were understood with time. That at the beginning of the century, the fundamental problem was trying to understand the atoms, an essential ingredient in understanding it was the realization that atoms are not fundamental, but composed of other particles, namely nuclei and electrons.

And just as the atoms were being understood, so then it was realized that there were certain patterns having to do with nuclei which, therefore, would lead you to believe that nuclei, in turn, are not fundamental, but they are in turn composed of something else and that turned out to be protons and neutrons.

And then so we entered—after the World War II—the phase we might call studying the particle physics, having to do with trying to understand protons, neutrons, and other so-called elementary particles like pions and other exotic things and I think it's in the last 20 or 25 years that there has been remarkable progress in this particular substructure of matter, which leads us to believe that maybe these things are made out of quarks and I think what we are entering right now is sort of a point in time where I think we have acquired a very good understanding of this particular substructure of matter and we are beginning to think that maybe the quarks themselves are not fundamental, but maybe we're just entering a new stage of matter just around the corner.

Dr. Wojcicki. So I think in the next transparency, I would like to address maybe a little bit of this question over here and trying to show you the progress that has been made in the last 20 years or so [slide 5].



PARTICLE PHYSICS EVOLUTION

	Atomic level	Particle level
1) Identification of basic building blocks	Elements (Hydrogen, Copper, Gold, ...)	Particles (Proton, pion, electron, ...)
2) Classification	Periodic Table of Mendeleev	Lepton and Hadron families
3) Understanding	Quantum Mechanics Electromagnetism Nuclei and electrons	Quantum Chromodynamics Glashow-Weinberg-Salam Quarks

SUCCESSES OF G-W-S MODEL

1) Unification of weak and electromagnetic forces

2) Different experiments explained in terms of 1 number

3) Prediction of existence of $W^{\pm}$ and Z^{0} particles

4) Prediction of $W^{\pm}$ and Z^{0} masses

SLIDE 5

OK, so I think what I would like to do is to basically indicate the progress on the particle level and probably the best way to do that is to show the analogy with the atomic picture. As I mentioned earlier, the atomic picture has been studied at the beginning of the century. I think one way maybe to understand this progress is to try to think of three separate steps in understanding any substructure of matter.

Dr. WOJCICKI. The first step is the identification of the basic constituents. The second one is the realization there are certain regularities and, therefore, classification of these constituents, and finally, is arriving at the ultimate understanding of these constituents and of the forces governing these constituents.

So I think in the atomic case, we first realized that there were different elements and each element was composed of atoms and these atoms were different. Then I think came the realization of the similarities between different elements, so-called periodic table that I think we all learned in introductory chemistry and then finally there came the understanding having to do with this periodic table and we realized that it's the laws of quantum mechanics and electricity and magnetism, together with understanding what the constituents are, lead us to the understanding and describing the spectroscopy of the atomic systems.

And I think a very similar progression has occurred in the last 20 or 25 years in the particle physics. First, I think we came to appreciate that there are certain "elementary particles," like protons, pions, neutrons, electrons and so on. Then later on, I think around the middle 1960's or so, there was realization that these things occur in families. This is the work that Murray Gell-Mann of Cal Tech received the Nobel Prize for some time ago, and then I think just in the last 5 or 10 years, I think we're coming to the understanding of the substructure of the particles. We realize that the so-called laws of so-called quantum chromodynamics, which is the theory of nuclear forces, and also Glashow-Weinberg-Salam model, which describes the weak interactions and electromagnetism—I mean, play the equivalent role here that quantum mechanics and electricity and magnetism play over here.

And also we, I think, learned to appreciate that the quarks are the constituents of the particles. So I think there has been this very much parallel progress, and I think we do believe that I think we have arrived very close to the ultimate understanding of the matter at the particle level. To a large extent, I think what has been very significant in the last 5 years was the so-called Glashow-Weinberg-Salam model and experimental tests and the verifications of this model.

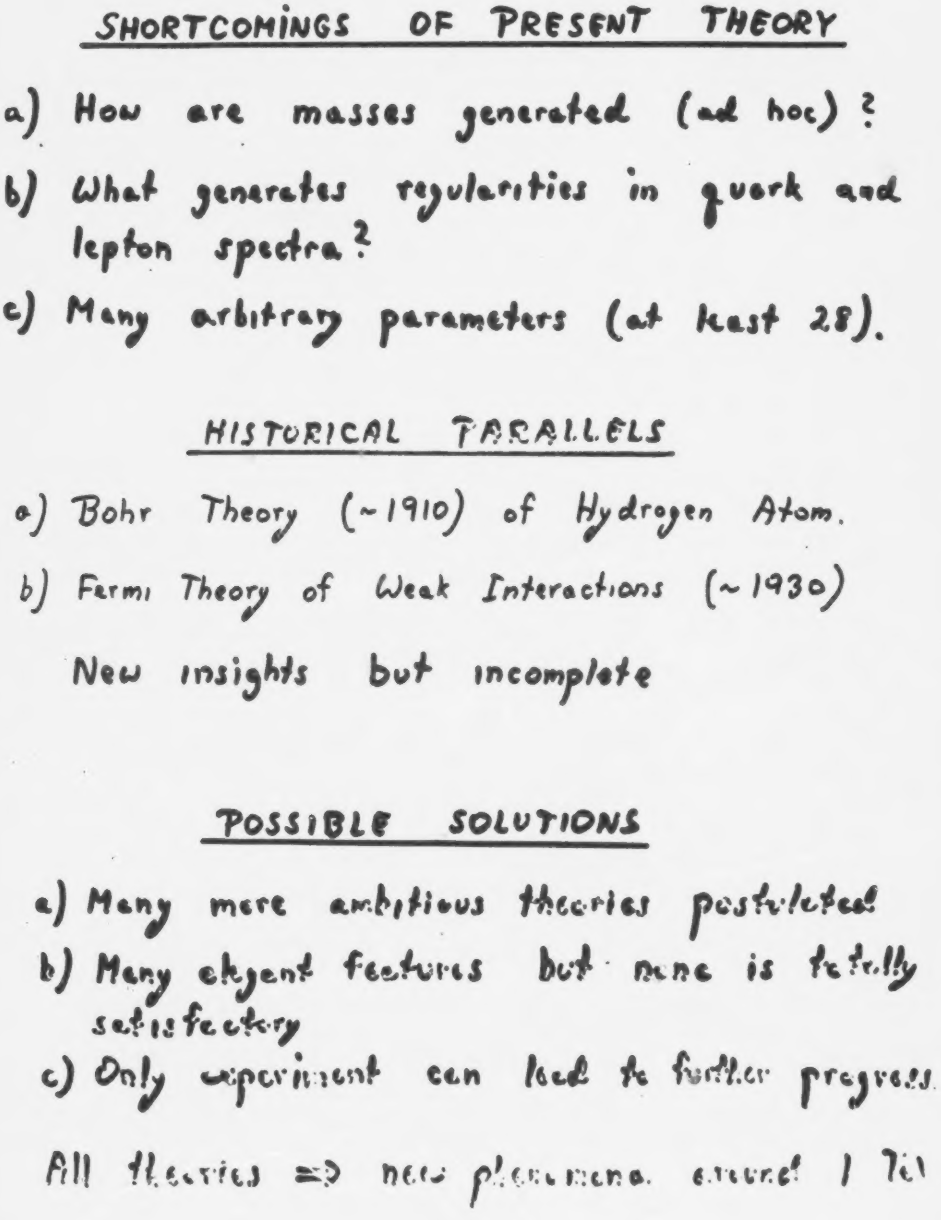
I think this is the work, again, that these three gentlemen have received the Nobel Prize for, I think, 3 years ago, in physics.

To mention some of the successes of the Glashow-Weinberg-Salam model, first of all, there's been a very intellectual—I mean, achievement of an intellectual goal of unifying what appears as two separate forces; namely, the weak and electromagnetic.

Second, there's been the ability to correlate a great deal of data in terms of essentially one single parameter. Finally, there's been a prediction of the existence of so-called weak intermediate bosons with masses considerably higher than the energies from which this prediction arose.

So I think intellectually, this was really a true achievement and I think, as you probably have heard in the last 3 or 6 months, those particles indeed have been discovered at CERN with the properties very much like those postulated by the Glashow-Weinberg-Salam model.

So I think the point that I would like to convey to you is that there have been a remarkable series of both theoretical and experimental successes and therefore, it's legitimate to ask the question, do we have an ultimate theory and I think the answer is probably no, and I think that's what we have sort—I mean, in the last few years, we have sort of realized that, first of all, there are certain unsatisfactory features of this particular theory [slide 6].



SHORTCOMINGS OF PRESENT THEORY

a) How are masses generated (ad hoc)?

b) What generates regularities in quark and lepton spectra?

c) Many arbitrary parameters (at least 28).

HISTORICAL PARALLELS

a) Bohr Theory (~1910) of Hydrogen Atom.

b) Fermi Theory of Weak Interactions (~1930)

New insights but incomplete

POSSIBLE SOLUTIONS

a) Many more ambitious theories postulated

b) Many elegant features but none is totally satisfactory

c) Only experiment can lead to further progress.

All theories $\Rightarrow$ new phenomena around 1 TeV

Slide 6

For one, there has been no explanation of where the masses come from. I mean, particles have masses; the quarks have masses; electrons have masses and so on. The—the weak intermediate bosons have masses. Where do these masses come from? Why are they so diverse? Why is one so much different from the other?

There is in the theory basically an ad hoc explanation, but I think most people feel that it's unsatisfactory and probably something much, much more fundamental exists.

There appear to be regularities, again, in the quark and the lepton spectra, in the same way that we had periodic table of elements, it appears that we have "periodic table of quarks and leptons," and therefore, that again leads us to believe maybe they're not fundamental. Maybe there is really something behind it.

Finally—which I think again is a very unsatisfactory theory—there are a lot of arbitrary parameters. I mean, all the masses, for example, are quite arbitrary. A lot of other arbitrary parameters, so we just don't believe that a theory with so many parameters can really be truly fundamental.

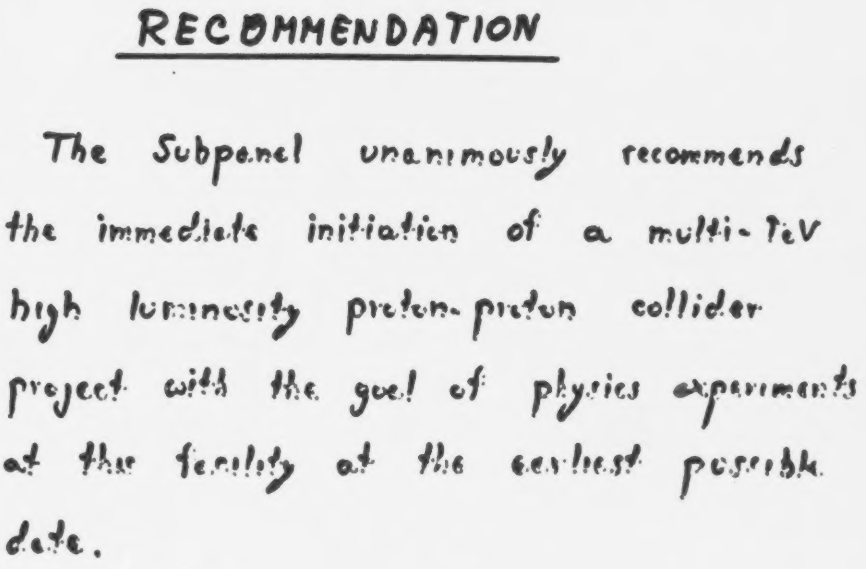
Now, I might just again peripherally mention that this is not uncommon, if you look at the history of physics, that sometimes a very important breakthrough comes through with a theory which does provide new insights, but it's not clearly fundamental. I mean, clearly there is something left in the theory. The theory is not totally satisfactory.

The Bohr theory of the hydrogen atom, which introduced quantum mechanics—I think it's really one example of it. Fermi's theory of weak interactions is another. I think both were important milestones in our understanding, but certainly not ultimate theories. There were clearly drawbacks.

So as a result, I think, of some of these arguments, I think lot of—there's been a lot of intelligent work done in the last 3 or 5 years, trying to explain some of these shortcomings and I think probably the best way to summarize that is to say that even though many new and rather good ideas have been brought forth, it's rather unlikely that any of these ideas really provide the ultimate answer and I think most people do agree that the ultimate answer can only really come from the experiment. We really need more experimental data.

The other point that I think is very important is that basically on other general grounds, most of those theories agree that the next crucial experimental frontier where I think you have to do the experiments—the matter that you have to investigate essentially is around 1 TeV energy scale.

OK, so I think it's these kinds of arguments—they have basically led to the first recommendation of the subpanel, namely, the initiation of a—of a multi-TeV high luminosity proton proton collider, which would have the capability of exploring this 1 TeV mass scale.



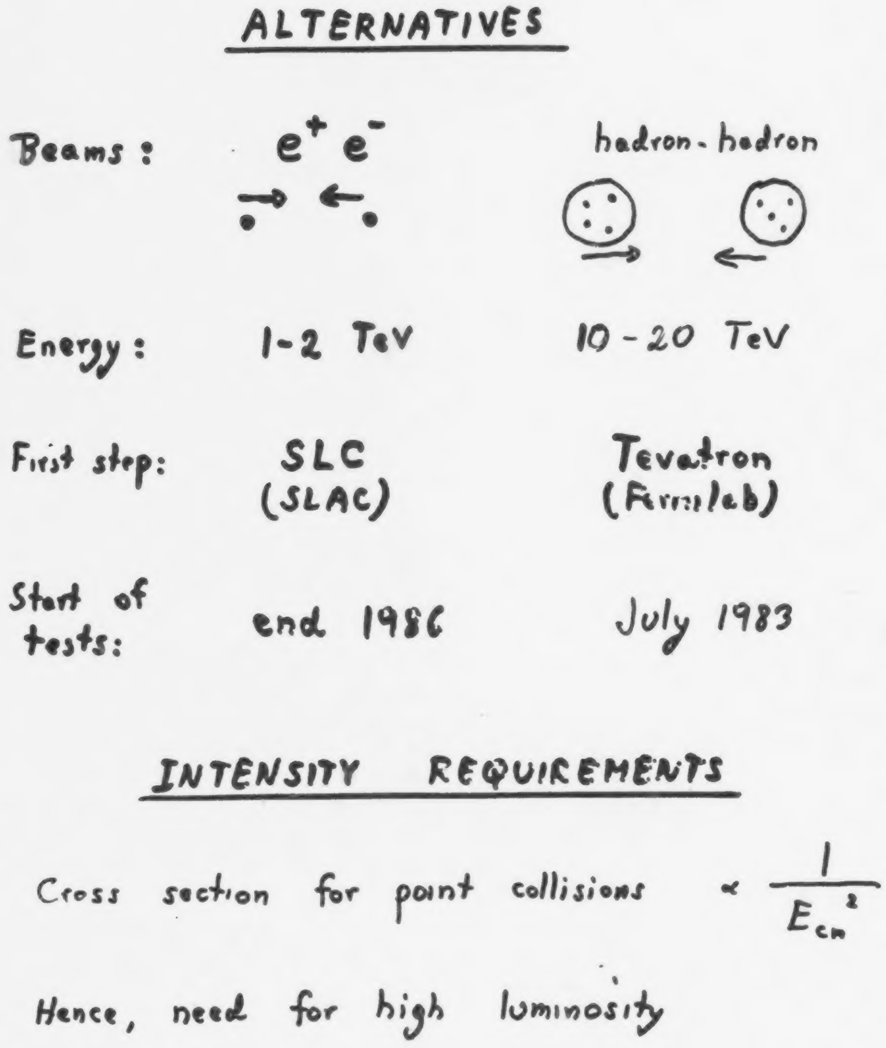
RECOMMENDATION

The Subpanel unanimously recommends the immediate initiation of a multi-TeV high luminosity proton-proton collider project with the goal of physics experiments at the facility at the earliest possible date.

SLIDE 7

Let me maybe take the next couple of transparencies to just say a few more details about this.

Dr. WOJCICKI. I think one—the first question that I think we have to ask is about the possible alternatives [slide 8]. In other words, do we explore this 1 TeV mass scale with the electron machines, like is being done at Stanford, or with hadron colliders, proton/proton colliders?



ALTERNATIVES

Beams:	$e^+ e^-$	hadron-hadron
Energy:	1-2 TeV	10-20 TeV
First step:	SLC (SLAC)	Tevatron (Fermilab)
Start of tests:	end 1986	July 1983

INTENSITY REQUIREMENTS

Cross section for point collisions $\propto \frac{1}{E_{cm}^2}$

Hence, need for high luminosity

SLIDE 8

Now, I think to understand this question, I think we have to bear in mind that what we really need is enough energy in the fundamental constituent center of mass system to create these new particles. In other words, that either, in terms of electron/electron collisions or quark/quark collisions, we have to have of the order of a few TeV to really create these particular particles.

Now, we believe, as Dr. Trivelpiece already mentioned, that electrons are fundamental, at least at the level of 10 to the minus 16 or 10 to the minus 17 centimeters. On the other hand, we know that the protons are not. We can think of them as basically bags consisting of marbles, whereas electrons are marbles themselves.

What we do when we collide two protons is to collide these two bags together, and what we are interested in is that two of the marbles in these bags collide and the rest of the marbles go whizzing by.

Therefore, clearly, the amount of energy for the hadron/hadron collision has to be larger than 1 or 2 TeV; just essentially larger by the number of constituents and that is set up roughly like a factor of 5 or 10. So therefore, if we go to the electron machines, we really need 1 or 2 TeV. If we go to the proton machines, we really need 10 or 20 TeV to be able to explore this 1 TeV mass scale.

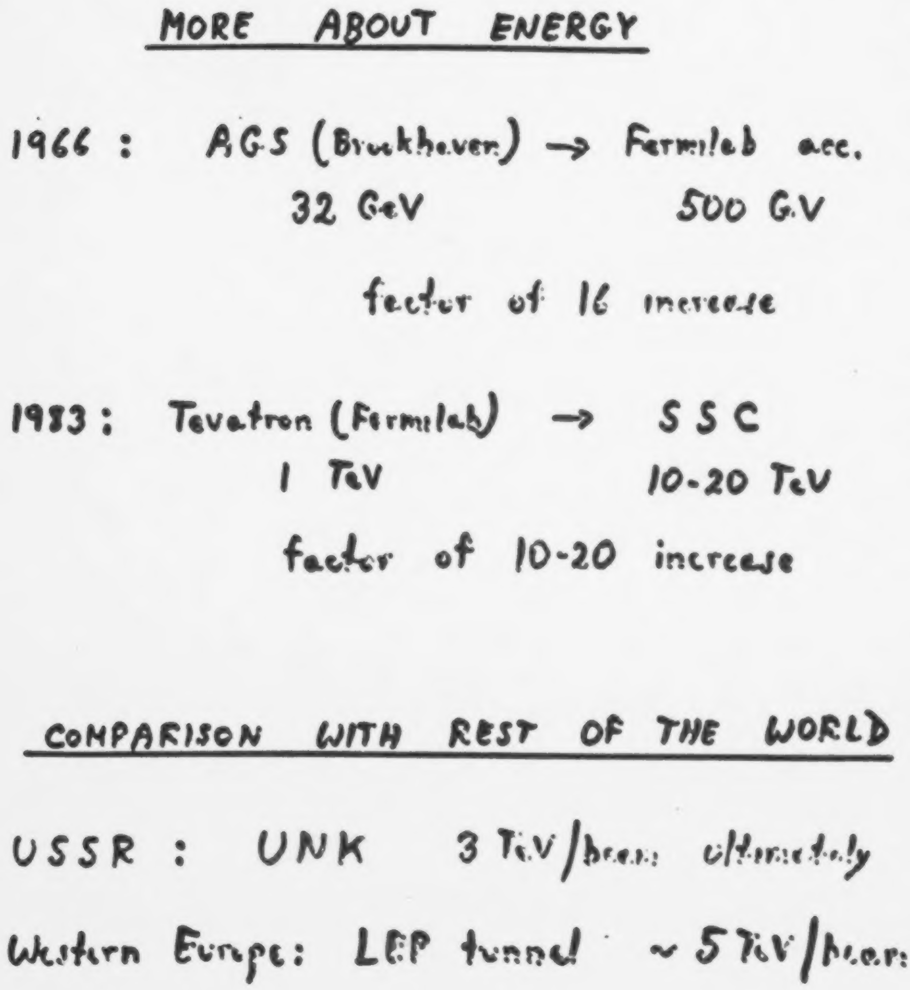
OK. Now this might argue for the electron machines. On the other hand, there is very important technical field which I think weighed heavily in the subpanel's deliberations and that is that we believe that the technology exists today for the hadron/hadron collider to achieve this goal, but it does not really as yet exist for the e plus-e minus machine.

I think the machine that's been recently approved for Stanford University is, you might say, the first step or the first step in the class of e plus-e minus, electron/positron machines, which will hopefully ultimately allow us to reach this kind of energy scale, but I think until the SLC works, and until one understands the problems, we feel that technology doesn't exist to really—to really do this. So I think that's basically the argument for the hadron/hadron collider.

Dr. Wojcicki. The other point that I think I would like to emphasize is that as we go to these high energies, we need very large luminosities, very large intensities.

Again, it's just a reflection of the fact that I mentioned earlier, that as we go to the higher and higher energies, the objects seem to look smaller and smaller. There is this one-to-one correspondence and, therefore, for them to collide on each other, we clearly need more of them. So I think the argument is that the intensity of the machine or the luminosity has to be quite high.

Dr. Wojcicki. I would like to maybe end this discussion about the SSC with this final transparency on the subject and that is, first of all, I would like to maybe put in perspective the energy scale of this machine [slide 9]. When the Fermilab accelerator was authorized—I believe in 1966—the Fermilab accelerator being eventually a 500 GeV accelerator, the highest energy proton accelerator in the world at that time was the alternating gradient synchrotron machine at Brookhaven and the Fermilab machine represented a step of roughly a factor of 15 or so in the energy.



MORE ABOUT ENERGY

1966 : AGS (Brookhaven) → Fermilab acc.
32 GeV → 500 GeV

factor of 16 increase

1983 : Tevatron (Fermilab) → SSC
1 TeV → 10-20 TeV

factor of 10-20 increase

COMPARISON WITH REST OF THE WORLD

USSR : UNK 3 TeV/beam ultimately

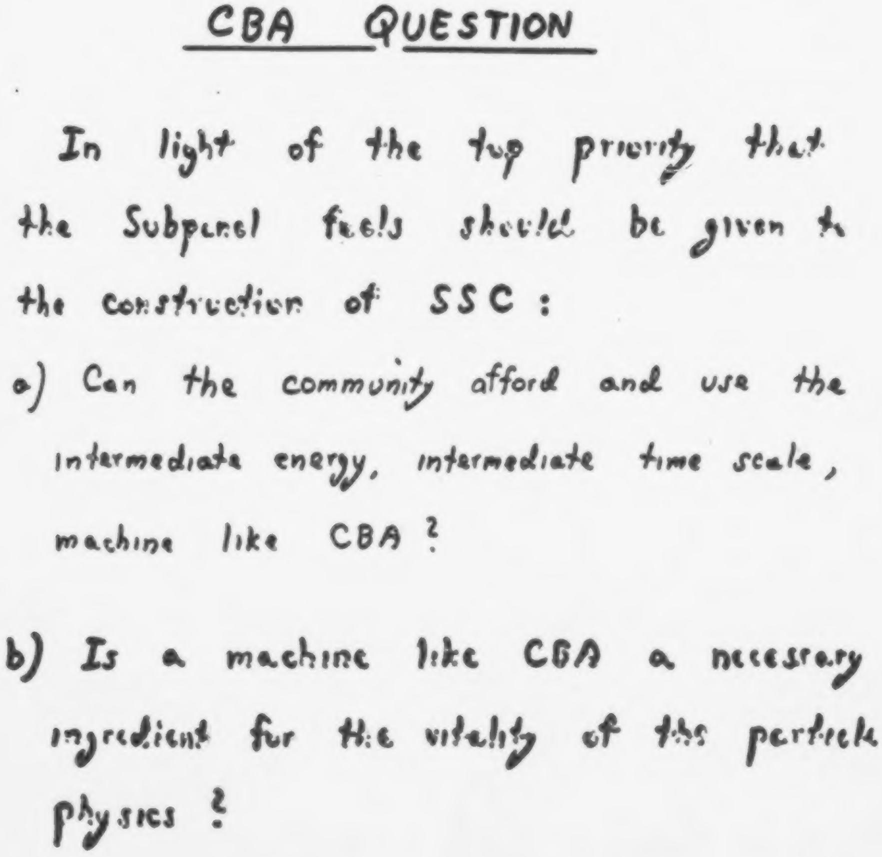
Western Europe: LEP tunnel ~5 TeV/beam

SLIDE 9

And I think we feel that the SSC represents roughly the same advance, at least in terms of the increase, the relative increase in energy as the Fermilab did over the AGS. So I think the argument is that it is not really totally off scale as far as what we're projecting and I think that has been projected and projected successfully in the past.

I think the other point that I should mention is what is being done around the rest of the world. There is a project just starting in the Soviet Union which eventually will end up with a 3 TeV per beam accelerator in some 10 years or so. At the same time, in Western Europe, there will be capability in the so-called LEP near Geneva to build probably around a 5 TeV per beam machine. Whether the Europeans will or will not do that, I don't know. I think to a certain extent, it depends as to what the Federal Government decides about the American—the proposed or recommended American project.

OK, so I think—that's as much as I would like to say about the SSC.



CBA QUESTION

In light of the top priority that the Subpanel feels should be given to the construction of SSC:

a) Can the community afford and use the intermediate energy, intermediate time scale, machine like CBA ?

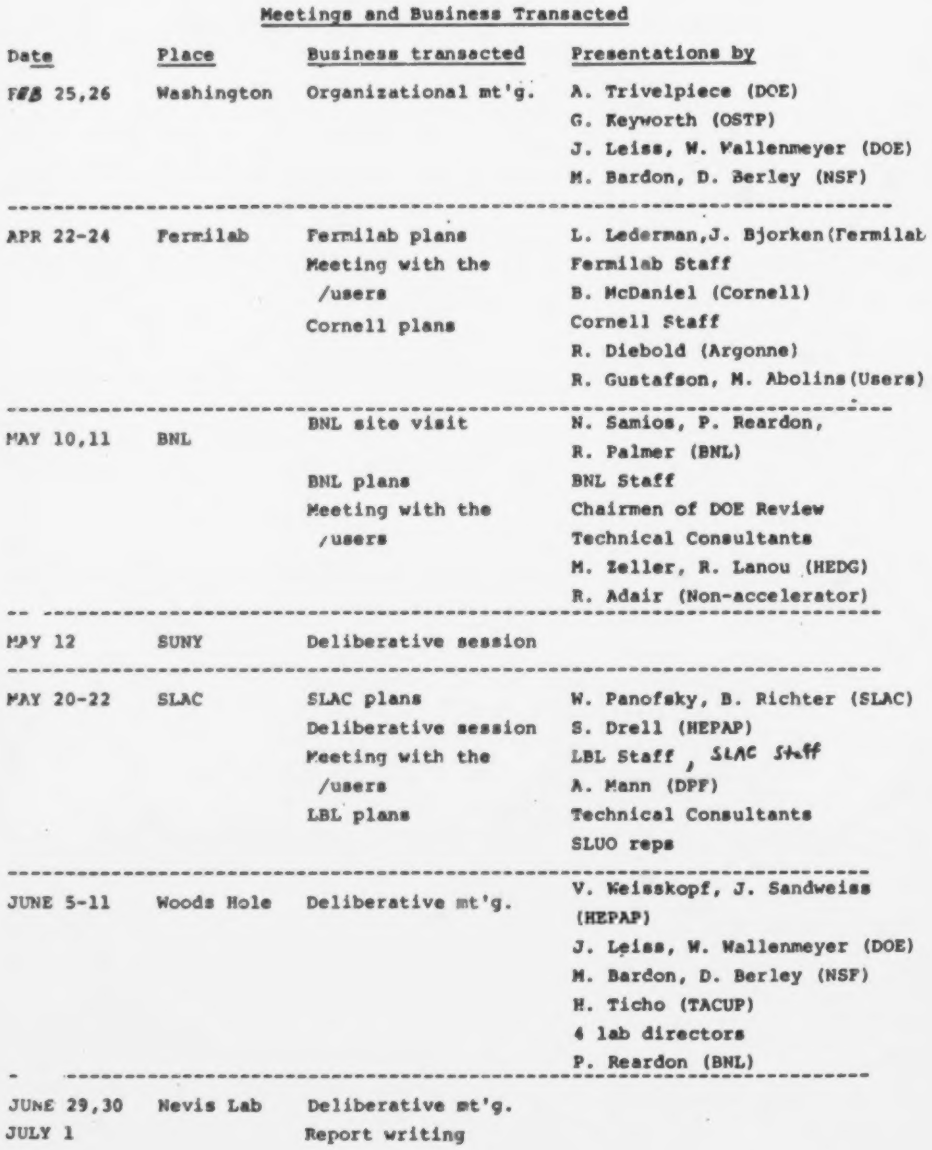
b) Is a machine like CBA a necessary ingredient for the vitality of the particle physics ?

SLIDE 10

Dr. WOJCICKI. Let me now turn to the CBA question, and I think, again, I would like to emphasize that at least in our slide 10 deliberations, we tried to look at the whole U.S. program and basically the question is, can one really—I think there was this very strong feeling that the ultimate energy machine is an important step, and the question is, can one at the same time afford to do both of those things in parallel?

So basically, you might say that if I were to characterize the question, I could maybe do it in those two specific points, namely, can the community thus—are there enough resources available; and second, is it an absolutely necessary ingredient to the vitality of the physics program on the intermediate time scale?

Just to maybe indicate to you very crudely the sort of the extent of the subpanel's deliberations, I try to indicate to you what the subpanel has done. It basically started at its first meeting in February. The final report was completed on July 1 for the HEPAP deliberations on July 11. The subpanel has visited the three ma—I'm sorry, three major laboratories [slide 11].



Meetings and Business Transacted

Date	Place	Business transacted	Presentations by
FEB 25,26	Washington	Organizational mt'g.	A. Trivelpiece (DOE) G. Keyworth (OSTP) J. Leiss, W. Wallenmeyer (DOE) M. Bardon, D. Berley (NSF)
APR 22-24	Fermilab	Fermilab plans Meeting with the /users Cornell plans	L. Lederman,J. Bjorken(Fermilab Fermilab Staff B. McDaniel (Cornell) Cornell Staff R. Diebold (Argonne) R. Gustafson, M. Abolins(Users)
MAY 10,11	BNL	BNL site visit BNL plans Meeting with the /users	N. Samios, P. Reardon, R. Palmer (BNL) BNL Staff Chairmen of DOE Review Technical Consultants M. Zeller, R. Lanou (HEDG) R. Adair (Non-accelerator)
MAY 12	SUNY	Deliberative session	
MAY 20-22	SLAC	SLAC plans Deliberative session Meeting with the /users LBL plans	W. Panofsky, B. Richter (SLAC) S. Drell (HEPAP) LBL Staff, SLAC Staff A. Mann (DPF) Technical Consultants SLUO reps
JUNE 5-11	Woods Hole	Deliberative mt'g.	V. Weisskopf, J. Sandweiss (HEPAP) J. Leiss, W. Wallenmeyer (DOE) M. Bardon, D. Berley (NSF) H. Ticho (TACUP) 4 lab directors P. Reardon (BNL)
JUNE 29,30 JULY 1	Nevis Lab	Deliberative mt'g. Report writing	

SLIDE 11

Dr. WOJCICKI [continuing]. Brookhaven, Fermilab, and SLAC.

In addition to that, it held two long deliberative meetings. It has solicited a great deal of testimony from the high energy community, has also used extensively the technical work that has been done in the preceding year by various workshops and other working groups and at the same time, has also tried to formally, before the subpanel, hear testimony from a large number of people that might be able to elucidate on some of these problems.

Now, unfortunately, I do not have the time really to delve deeply into all of the issues that were discussed by the subpanel and to present to you all of the arguments for and against and how the decision was reached, but I think I would like to maybe just illustrate some of the issues that were discussed, for no other reason than to indicate the complexity of the problem and some of the issues that we have tried to—tried to grope with.

So I think I have tried to indicate them here. Clearly the overriding issues were the physics—I mean, what could the CBA do? I mean, 4 years from now, basically what is its physics potential?

Colliding Beam Accelerator (CBA)

The Subpanel has examined this issue by looking at a number of different points.

A reasonably complete summary of these is:

Physics:

- Discovery potential
- Programmatic studies

BNL Capability:

- Technical questions
- Middle level management
- Costs to complete

Importance of Diversity:

- 6 Interaction Regions
- Polarized beams, Asymmetric beams, Heavy Ions
- Luminosity, Detectors

Interaction with SSC:

- Financial Considerations
- Manpower: physicists, accelerator specialists
- Detector Development

SLIDE 12

Dr. WOJCICKI. We tried to look at the technical capability of Brookhaven, both in managerial and the technical and the financial, to determine if the project could be done as advertised. We tried to understand the importance of the diversity, I think. Everybody realized that CBA would provide new facilities, new capabilities and the question was really how important they were? How should they be traded off against some of the other things?

Very important, I think, was the issue of interaction with the super collider; namely, both from the point of view of financial considerations, from the point of view of manpower considerations, from the point of view to what extent CBA is necessary as an intermediate step for the ultimate machine.

So I think those were some of the issues. There were others.

Manpower question:

1987 - 1990 period
early 1990's

Different possible SSC scenarios

Completion by first half of 1990's
Delays and/or Difficulties

Interaction with other programs

AGS phaseout
Utilization of existing machines

Timescale vs. other similar machines

Tevatron I, $Sp\bar{p}S$	hadron-hadron
SLC, LEP	e^+e^-

Credibility and Consistency

Previous Woods Hole Panel recommendations
Questions of fickleness; redirection of programs

"Politics" and Sociology

Interaction with Federal Government
Unity of High Energy Community

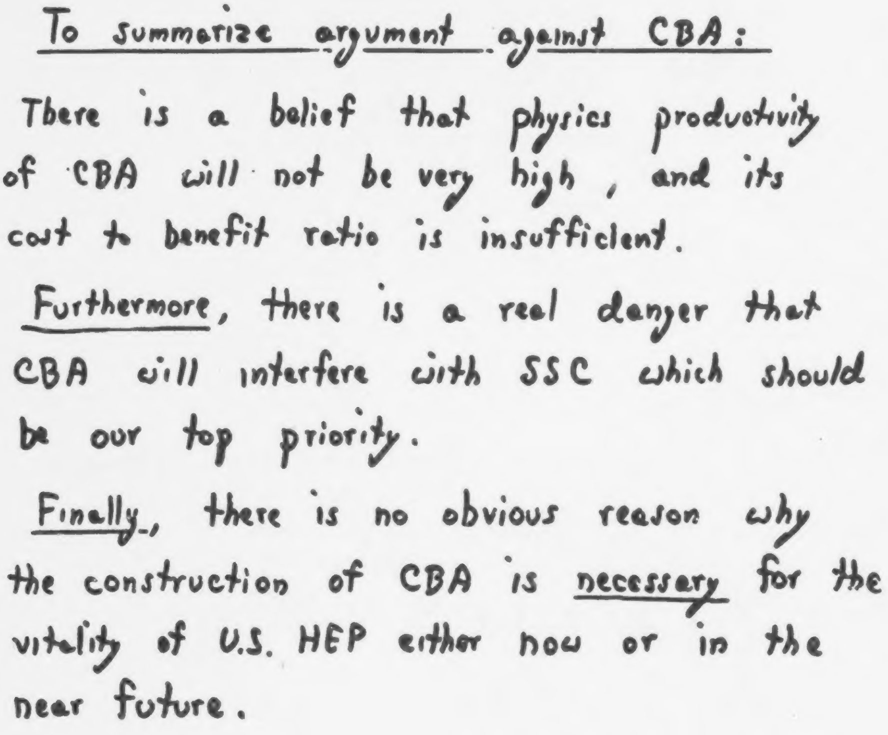
SLIDE 13

Dr. WOJCICKI. We have discussed the questions of manpower; do we have enough technical manpower in the country right now; what happens if we do not build the CBA; I mean, will there be sufficient opportunities to keep the field vital in the early 1990's?

We tried to appreciate the fact that the SSC is just something—an idea in the minds of the high energy physicists. There are a lot of potential question marks and we tried to examine, you know, what the decisions should be under both the most optimistic and the most pessimistic scenarios.

We tried to understand the interaction with the other high energy programs in this country and also the physics interaction with some of the other machines, both hadron/hadron and e plus/e minus, which could potentially do similar kinds of physics.

We tried to really understand to what extent we are totally departing from the previous subpanels and to what extent things have changed qualitatively that we should have this redirection. And finally, we tried to understand politics because we're not only talking about national politics, but also high energy politics and the sociology. I mean, we were trying to, act in the most responsible possible way, but at the same time to—I think we were very much worried that the project is big enough that the whole high energy community has to be behind it.



To summarize argument against CBA:

There is a belief that physics productivity of CBA will not be very high, and its cost to benefit ratio is insufficient.

Furthermore, there is a real danger that CBA will interfere with SSC which should be our top priority.

Finally, there is no obvious reason why the construction of CBA is necessary for the vitality of U.S. HEP either now or in the near future.

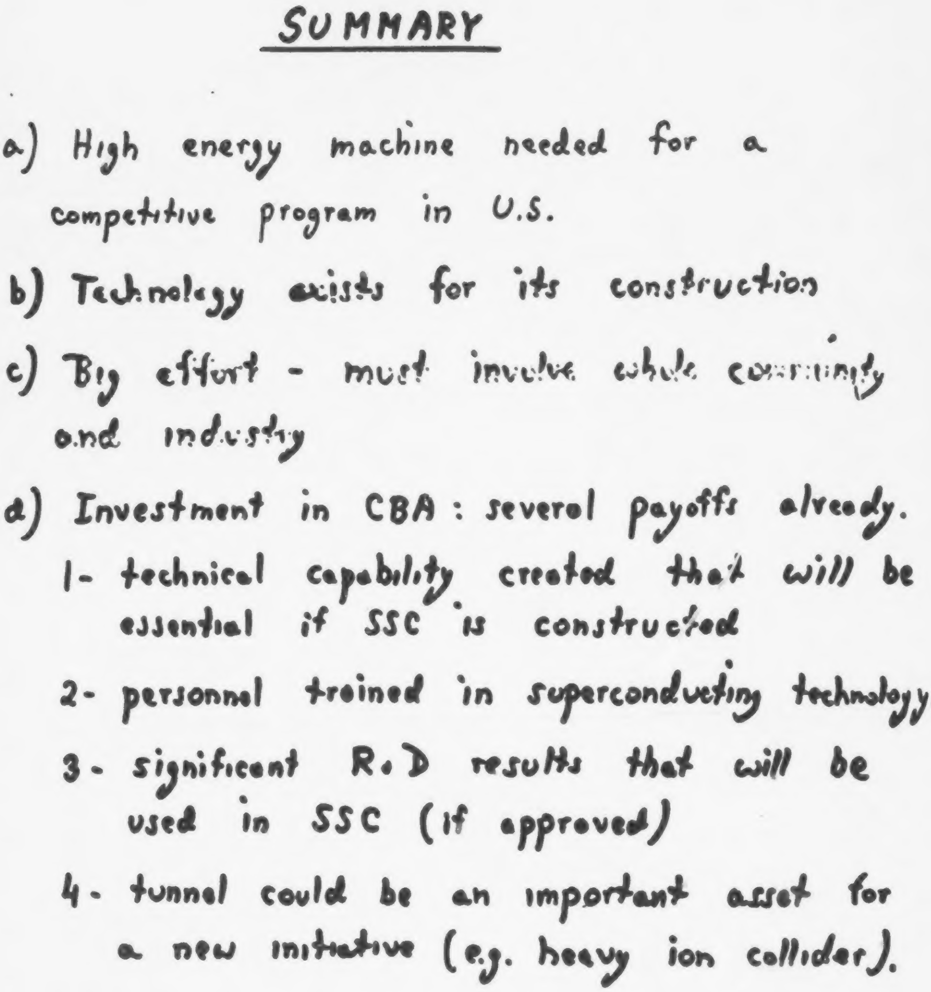
SLIDE 14

Dr. **Wojcicki**. OK. Now, let me maybe take two more transparencies to summarize. I think one—as I said, I really cannot do justice to all of the arguments. I think it would just too—take too long, but I will try to maybe reduce the argument to basically two or three comments and I think I've done this here and I think there are basically three points to it. Let me maybe just read it.

I mean, first of all, a belief that the physics productivity of CBA will not be high and I think again that should be taken in the context of some of the other projects, the fact that both the Tevatron in this country and the machine at CERN will have been on the air for about 4 years and, therefore, some of the cream of the crop will have been done.

Second, I think there was a feeling that the CBA will interfere for a variety of reasons with the superconducting collider and finally, we felt that, especially if the other facilities are utilized to their utmost, that the vitality of the field can be preserved—I mean, if the—should the SSC be approved.

OK, so I think—let me maybe summarize with—on this transparency.



SUMMARY

a) High energy machine needed for a competitive program in U.S.

b) Technology exists for its construction

c) Big effort - must involve whole community and industry

d) Investment in CBA: several payoffs already.

1- technical capability created that will be essential if SSC is constructed

2- personnel trained in superconducting technology.

3- significant R.D results that will be used in SSC (if approved)

4- tunnel could be an important asset for a new initiative (e.g. heavy ion collider).

Slide 15

Dr. Wojcicki. And I think I tried to make the point—I think these are sort of somewhat, you might say, personal and subjective comments. I think essentially the whole subpanel agreed that the high energy machine is really necessary for a competitive and vital high energy program in this country.

I think it's also agreed that I think the technology exists today, the successes at Fermi with the Tevatron, I think are a good indication of that and I think, third, that it's really a big effort. I don't think anybody kids themselves about that and I think it's realized that the whole high energy community and industry has to be involved very heavily in this.

I think finally I should maybe say a couple of words about the CBA. Clearly the question that I think has to be raised—and I think we have tried to grapple with—is the investment in the CBA facilities. It's something that you do not want to give up and I think you have to understand, I think, this kind of a decision and I think the point that I would like to argue that it's not—that you're just writing off a certain investment, because already, I think there have been a lot of technical payoffs from the work that has been done at Brookhaven on the CBA.

For example, there's been a very high level technical capability, superconducting capability, created at Brookhaven, having to do with magnet testing facilities, the large-scale refrigeration and so on, which I think will be absolutely essential for the SSC, should the SSC be constructed—approved and constructed.

I think a very important point has to do with people. I mean, clearly, training scientific personnel is not a trivial thing and I think the CBA project over the last few years has contributed greatly to increasing the manpower pool, people who are very competent in this particular field.

Third, I think there have already, been significant, results from this program which, again, would be essential to the SSC, should the SSC be approved, and I think finally I might add that I think it—there's been a lot of interest expressed in the last few months in the possibility of using the CBA tunnel for some other purpose and I think the heavy ion collider, I think is one area that I think several—a lot of nuclear physicists and high energy physicists are very much interested in.

Well, Mr. Chairman, that concludes my testimony. I apologize if I took too much time and I'll be happy to answer any questions.

[The prepared statement of Dr. Wojcicki follows:]

SUMMARY OF STANLEY WOJCICKI'S STATEMENT.

The recent years have seen a remarkable series of successes in the field of particle physics. Major theoretical advances have resulted in creation of a formalism that not only unifies the weak and electromagnetic forces, but at the same time is capable of making a variety of rigorous experimental predictions. On the experimental front, the last five years have provided crucial experimental tests of this theory, all of them in remarkable agreement with its predictions. This experimental work has culminated recently in the discovery in Europe of the postulated W and Z bosons, with properties in full accord with theoretical postulates.

Even though those theories have been greatly successful to date, there is one important aspect of the theory that has not been tested so far, namely the part that has to do with the mechanism of generation of particle masses. From the aesthetic point of view, there is a feeling that this aspect of the theory is least satisfactory and very likely will need to be modified. On very general theoretical grounds one can argue that these very fundamental questions can only be solved experimentally by probing energies in the 1 TeV region.

There is strong agreement in the particle physics community that this mass problem is the crucial question facing high energy physics in this decade. In addition there is also an agreement that today we already possess technology which make feasible construction of a 10-20 TeV hadron-hadron collider that will allow us to achieve the necessary energy in the quark-quark subsystem. Accordingly, the HEPAP subpanel has recommended construction of such an accelerator as a number one priority for high energy physics.

The subpanel has debated extensively the question of the Colliding Beam Accelerator (CBA) at Brookhaven. A large number of relevant factors were discussed and not all the members of the subpanel reached the same final conclusions even though there was a reasonable agreement on basic facts. The majority view, however, was that the high energy community does not possess the resources necessary to proceed with the CBA and at the same time embark on the 10-20 TeV accelerator as expeditiously as necessary. Thus, in spite of the general consensus that the BNL has solved all of the important technical problems that have plagued the CBA in the past, the subpanel recommended that the CBA project not be approved.

A WRITTEN STATEMENT BY STANLEY G. WOJCICKI TO THE SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS, PRESENTED ON OCTOBER 19, 1983

Particle physics can probably be described most directly as that science which concerns itself with the following two questions:

(a) What are the fundamental constituents of matter?

(b) What are the forces that govern the interactions between these constituents?

In addition, in the last decade a very exciting intellectual development has been the apparent ability of particle physics to elucidate our knowledge about the beginning of the universe. Cosmologists and particle physicists have been able to make a close connection between the conditions prevalent in the early universe (the first fraction of a second after its birth) and the situation which exists in very high energy particle collisions. The relics remaining from that early stage of the universe may clarify some of the theoretical particle physics problems; similarly high energy laboratory experiments may shed light on the details of the "Big Bang" i.e., the origin of our universe. This situation is well summarized in the following quote taken from the Report on Astronomy and Astrophysics in the 1980's (the Field Report):

> *"The discoveries of our generation have brought us to the threshold of a revolution in physical thought, in which the properties of elementary particles may hold the key to understanding the early history of the Universe..."*

The questions of the composition of matter and the origin of the universe have occupied mankind since the beginnings of recorded history, and it is hard to deny the fundamental nature of this intellectual challenge. As an example, we have the two opposing views of the ancient Greeks on the subject of basic constituents of matter: Aristotle's picture of four basic elements and the atomic viewpoint of Democritus.

Whereas these early speculations can be classified as philosophy, the experimental investigations regarding the composition of matter did not start until more modern times, around the beginning of the 17th Century. There followed, during the next 3 centuries, the pioneering work in the field of chemistry which provided the first understanding of the laws of chemical reactions and gave the first evidence for the existence of various elements.

Particle physics, as it is known today, did not enter into these investigations until the beginning of the 20th Century. Subsequently, we have witnessed a profound change in our understanding of matter as it is explored on a successively finer scale. I shall present a brief summary of these developments, as a convenient way to introduce and discuss some of todays questions.

In this century physicists have unraveled three different levels of the structure of matter corresponding to atoms, nuclei, and elementary particles. Each one of these levels can be characterized by its own distance scale, i.e., size of the substructures, and by the force which governs the system. Thus the typical distance scale of the atoms is of the order of 10^{-8} cm and it is the electromagnetic force which is responsible for binding the atomic constituents (nuclei and electrons) to form the different atoms. What has happened historically, in each of the three cases enumerated above, is that just as a full understanding of one structure was being reached, new questions arose concerning regularities that began to appear at even shorter distances. Thus, just as the atom seemed finally to be understood in the 1930's, so new discoveries lead to the realization that the heart of the atom, the nucleus, is itself a complex object, which is composed of even smaller constituents. And thus the field of nuclear physics was born.

In a similar way, a reasonable understanding of nuclear structure had been reached by the mid 1950's, but then the observations of new "elementary" particles in cosmic rays, the regularities and similarities they displayed, and the great proliferation of these new particles, led to the realization that there must exist a more fundamental level of matter.

In the last decade we have witnessed an apparent culmination of the study of this particular spectroscopy. The elementary constituents appear to be divided into two general classes: the leptons and the quarks, both apparently structureless down to distances as small as 10^{-16} cm. The former include particles like the electron and the elusive neutrino; the latter appear to be unobservable, but a variety of both static observations (i.e., regularities in spectroscopy of particles) and dynamic data (i.e. scattering experiments) allow us quite precisely to pinpoint their properties.

In addition, the theories predicted the existence of another set of particles, both massive and massless, called the gauge bosons, which are responsible for generating the known forces. Thus the well known photon is the carrier of electromagnetic force; the weak force was postulated to be due to a set of heavy bosons, namely $W^{\pm}$ and Z°; the strong force was believed to be mediated by a set of bosons called gluons; and finally gravity could be understood as transmitted by another massless particle, the gravitino.

There are very strong reasons to believe that gluons have indeed been observed, initially in the experiments at the DESY Laboratory in Hamburg, West Germany. But the real crowning achievement of this theoretical edifice has been the recent observation at CERN in Geneva, Switzerland, of the W and Z bosons. Several years ago, a trio of physicists, S. Glashow, A. Salam, and S. Weinberg independently proposed a theory which not only unified the weak and electromagnetic interactions in one simple formalism but simultaneously made some very strong predictions. From the point of view of theoretical significance, this work, if shown correct, would rank comparably with Maxwell's theory at the end of the last century which unified electricity and magnetism.

The list of predictions made by the Glashow-Weinberg-Salam model is quite impressive. Let me just enumerate two of them. First it provides a framework in which a variety of diverse phenomena can be understood in terms of a single parameter, the weak angle Θ_W. These phenomena include:

a) neutrino electron scattering over a very wide energy range

b) neutrino proton and neutrino neutron scattering

c) e^+e^- annihilations into leptons and hadrons

d) polarized electron deuteron experiments

e) atomic physics experiments

Second, based on the value of Θ_W derived from the above data, the theory predicted the existence and the masses of the W and Z bosons. As an indication of the predictive power of the theory, one should mention that the model predicts the mass difference between the W and Z bosons at the percent level. This is even more remarkable when we realize that the masses of the W and Z bosons are several orders of magnitude larger than the energy scale of experiments from which the predictions were obtained.

The main thrust of particle physics during the last ten years have been the experimental verifications of this theory. Both the lower energy experiments, and the recent results from CERN on the $W^\pm$ and Z° bosons, show perfect agreement with the Glashow-Weinberg-Salam scheme.

Does this mean that particle physics is finished and we have achieved the ultimate goal? Indeed, a few years ago there were such opinions expressed. However, more recently there has been an growing expectation that a great wealth of new physics lies just "around the corner". I would like next to elaborate on that point.

Essentially all present tests of the weak interaction theory concern the symmetry nature of this theory. The symmetry postulated for the theory predicts specific values for different coupling constants (strengths of interactions) and the experiments have verified the validity of these predictions. But there is another aspect of the theory, which as yet has not been subjected to any stringent experimental test. That aspect has to do with the question of how elementary constituents receive their masses, e.g. what determines that the mass of the electron is about half an MeV and that of the Z° boson almost 200,000 times heavier? The theory does attempt to provide a mass-generating mechanism, the so called Higgs mechanism, but most theorists find serious conceptual difficulties with that aspect of the model in addition to its unesthetic nature of being somewhat *ad hoc.*

One could make an argument that the status of the Glashow-Weinberg-Salam model today finds itself in a similar position as did the Bohr model of the Hydrogen atom and Fermi's 4-point Weak Interaction theory shortly after their conception. Both of those theories were remarkably successful in their predictive power and they both provided revolutionary new insights into important physical questions. However both were obviously incomplete; each had its own "Achilles Heel". The questions of electromagnetic radiation in the case of the Bohr atom and of the high energy divergences in Fermi's interaction were serious enough shortcomings to indicate that these two formalisms were only an initial approximation to a more complete theory. It is believed that the mass generation question could easily present the same problem to the unified weak-electromagnetic model.

One can make additional arguments that we do not have the ultimate theory of fundamental constituents of matter. If we look at the systematics of leptons and quarks we see a remarkable number of regularities. Thus the number of lepton and quark "families" are identical (three in each case). The obvious question is why, to which no answer is forthcoming from the G-W-S model. The quarks have charges 2/3 and 1/3 in units of electronic charge; the lepton charges are 0 and 1. Again we ask why does this regularity exist and again we know of no answer. In the past, regularities in the classification of "fundamental" constituents always indicated some underlying principle and another substructure of matter. Thus the regularities of Mendeleev's periodic table could only be explained with the advent of quantum mechanics and the structure of atoms led to the nuclei and electrons. The safe guess is that the history will repeat itself here.

In addition, we can point to a minimum of 28 constants, i.e. free parameters, that have to be fed into the present theory. On pure esthetic grounds, we can argue that any theory with so many free parameters simply cannot be a truly fundamental one. Finally we should point out that the simplest theory that appeared to be able to answer some of these questions and at the same time include the G-W-S model as its ingredient, the so called SU(5) of Georgi and Glashow, appears to be ruled out by the recent experimental data on proton decay. There are thus good reasons to believe that we are still somewhat away from the ultimate answer about the basic constituents and forces of matter.

To summarize, a remarkable progress has been made in the last two decades in understanding the spectroscopy of elementary particles. The basic ingredients of that understanding are the quarks and leptons as the basic constituents and the Glashow-Weinberg-Salam model as the theory of weak and electromagnetic forces. But strong indications exist that we do not have the ultimate theory and that a whole new substructure remains to be discovered. I would like next to discuss how we can hope to answer some of the questions that we discussed above.

I have already discussed how, with the passage of time, we were able to examine the constituents of matter at smaller and smaller distances. Thus typical atomic sizes are about 10^{-8} cm; nuclear sizes about 10^{-12} cm, and dimensions of "elementary" particles, e.g. proton, pion, etc. about 10^{-13} cm. Going down even further, we know that if quarks and leptons are composite, i.e. have structure, then that structure must be smaller than 10^{-16} cm. What I need to add here, is that to probe smaller and smaller distances, we need to use higher and higher energies. This is a simple manifestation of quantum mechanics which gives an inverse relationship between the energy of a particle and the wavelength associated with that particle. Thus this discussion argues that if we are to study distances smaller than already explored we must go to higher energies. The question is, are there any theoretical guidelines which indicate the desired energy scale? The answer is yes, and the energy goal indicated is in the vicinity of 1 TeV (10^6 MeV).

There are many theoretical models which attempt to answer at least some of the questions which have not been satisfactorily explained by the G-W-S picture. They all incorporate the G-W-S model as an essential ingredient of the overall picture but go a step further in trying to generate a deeper understanding of the structure of matter. Present space does not allow me to describe in detail any of these theories and so I shall limit myself to a few brief comments:

(a) Grand Unified Theories (GUTS) attempt to extend the weak-electromagnetic unification to the strong (nuclear) forces.

(b) The supersymmetic theories (SUSY), postulate a symmetry between half-integral spin particles (fermions) and integral spin particles (bosons).

(c) Technicolor theories postulate the existence of new quarks and new dynamics around the 1 TeV mass scale.

(d) Family symmetries propose the existence of a symmetry which links the different lepton and quark families.

(e) Left-right symmetric theories propose the restoration of left-right symmetry at high energies.

(f) Compositeness theories postulate sub-quark and sub-lepton constituents which will exhibit themselves at distances less than 10^{-16} cm.

This is a wide spectrum of theories. None of them is completely satisfactory and ultimately the correct theory probably contains only some of the elements of the models described below. However, they essentially all agree on the need for new phenomena around the 1 TeV region; thus it is only the exploration of that energy region which can enable one to make further progress in this field. In this context, it might be worthwhile to quote from a summary talk by L. B. Okun, the distinguished Russian theoretical physicist, at the conclusion of the 1981 Bonn (West Germany) International Conference on Particle Physics:

> *"It is evident that our theoretical picture of the origin of particle masses still lacks an important clue, a new theoretical idea, a new principle. I doubt whether this principle can be discovered by pure theoretical insight without a new experimental breakthrough."*

These are basically the outlines of the theoretical arguments that have led to the 1983 Woods Hole Subpanel's recommendation that the construction of a 10-20 TeV hadron-hadron collider should be the top priority of high energy physics in this country in this decade. In what follows I shall discuss some of the essential points in a little more detail.

Large numbers of models predict the existence of new phenomena at energies lower than 1 TeV even though the characteristic new mass scale is 1 TeV. Nevertheless, most people agree that the only sure way to get a proper understanding of these new interactions is to be able to explore the 1-3 TeV mass region. It is important to emphasize that we need this energy in the center of mass system of the "elementary" constituents, i.e. either the quark-quark system or the lepton-lepton system. Thus an accelerator which collides 2 TeV electrons on 2 TeV positrons would generate the required 4 TeV in the center of mass and could probably do the job. One the other hand, protons are not elementary but composed of quarks and gluons. Thus to generate 2 TeV on 2 TeV quark-quark collisions with any reasonable frequency we need considerably higher proton energies (a factor of 5-10 is a rough estimate). Thus a proton proton collider would need an energy of at least 10 TeV in each beam.

A legitimate argument can be made for the advantage of an e^+e^- collider, based on its "cleanliness", since in hadron-hadron collisions the characteristics of the quark-quark interaction are somewhat obscured by the presence of other hadron constituents. On the other hand, a stronger argument can be made for the hadron hadron collider, based on the much more advanced state of the art in that technology. The Tevatron accelerator, recently brought into operation at Fermilab, can be thought of as the prototype of the proposed 10-20 TeV Superconducting Super Collider (SSC). Its highly successful initial performance lead us to believe that superconducting accelerator technology has come of age and the SSC represents a natural extension of that technique.

In contrast, it is clear that a circular e^+e^- collider in the 1 TeV range is economically and technically not feasible due to its enormous power consumption (resulting from synchrotron radiation whose intensity increases as the fourth power of the energy of the stored beam). Hence there is a need for development of new technology, specifically that of head on linear colliders. The first machine of this kind, the SLAC Linear Collider (SLC), is being built at SLAC and is projected to yield 50 GeV on 50 GeV e^+e^- collisions by the end of 1986. It was the subpanel's opinion that until the first results from that very important accelerator development project become available, it would not be prudent to embark on a more ambitious project relying on this as yet unproven technology.

Thus one of the main recommendations of the subpanel, namely the construction of a high energy, high luminosity, hadron-hadron collider on a rapid time scale was based on:

(a) the physics arguments for the need to probe the TeV mass region

(b) the belief that this goal can be achieved by relatively modest refinements of the existing technology.

Regarding the second point, one might mention that the increase in energy between the Tevatron and the SSC is essentially identical (i.e. a factor of 10-20) as that between the AGS at BNL (the highest energy accelerator in existence at the time Fermilab was proposed) and the Fermilab 500 GeV accelerator.

The subpanel did not have the time or capability to examine a large number of more detailed questions regarding the proposed collider. It is hoped that they will be studied in depth during the next couple of years, should the Federal Government accept the HEPAP recommendation regarding the SSC. These questions include the precise design of the machine, the superconducting technology that should be used, detailed cost estimates, the fixed target option, advantages of a new site vs. existing laboratory, and detailed questions of management structure, both for the research and development stage and for the construction phase. The subpanel did feel that a minimum of 3 to 4 years of intensive R & D is necessary to obtain a magnet design that meets the required cost and reliability criteria.

The subpanel was fully aware of the large scale of this proposed project, and hence its inherent difficulties. There was a realization that a project of this magnitude can succeed only if it represents a full commitment of the whole US high energy community; if maximum, and optimal use is made of the capabilities of all the national laboratories; if there is a large scale involvement by the university groups; and if good use is made of the industrial expertise in the relevant areas. A new approach will be required to provide the best possible management structure for all of the diverse components that will be involved in this project.

I would like to turn next to the second major recommendation of the subpanel, namely not to proceed with the construction of the Colliding Beam Accelerator (CBA) at BNL. As stated in the report, that decision represented the view of the majority of the Subpanel, in whose deliberations

> *"There were areas of agreement both about facts and about the choice of key issues on which to base a decision, but in judging these issues and the weights to be given them, Subpanel members came to different conclusions".*

Based on the letters from the particle physics community written to the Subpanel, as well as the many private conversations held with the practising high energy physicists, I am personally convinced that this split vote reflected fairly well the divided opinion of the community on this very difficult issue. On the other hand, I also believe that the great majority of the community believes that a hard decision had to be made, a decision that could not satisfy everybody, and now that the subpanel recommendation was endorsed by HEPAP, the community is willing to support it.

I shall try to enumerate briefly some of the points that had been discussed in connection with the CBA. Space and time will not permit me to give the detailed arguments that the subpanel went through, but I hope that this enumeration of the arguments will at least convey the complexity of the issues involved. I should also emphasize that all the five recommendations of the Subpanel have to be considered together *in toto*. Thus the arguments about the CBA, both pro and con, have to be viewed in the perspective that all the Subpanel members agreed that the rapid construction of a high energy hadron collider is absolutely essential to a forefront US particle physics program.

a) The physics potential of the CBA was clearly one of the most important issues discussed. The subpanel tried to estimate the "discovery potential" of this machine as well as its ability to contribute to the more programmatic studies.

Both of these factors were discussed in the context of other accelerators and storage rings and their time scales. Thus the CERN $\bar{p}p$ ring, the Tevatron at Fermilab, and the SLC at SLAC have or will have capabilities to do some of the same kind of physics as the CBA. Clearly the earlier completion dates of other accelerators are an important advantage; the CBA could hope to recoup some of this edge by virtue of its high luminosity.

b) BNL capability to finish the job on the time scale proposed and within the budget proposed. The subpanel felt that these goals were achieveable but some worries were expressed about the extensive amount of recruiting needed for middle level management.

c) Diversity of the CBA and of its future options. The subpanel was aware of the six potentially different interaction regions at CBA, its capability to operate with beams of different energies, and the relative ease with which polarized beam interactions could be achieved. Furthermore, the subpanel did discuss the option of extending the CBA physics domain to heavy ions sometime in the future, and did realize that this could be done at relatively modest cost once the CBA was built and operating in the *pp* mode.

There were, however, serious questions expressed whether the CBA diversity can indeed be properly explored within the budget postulated, and whether the high energy community has sufficient manpower resources to utilize its full capabilities. It was also argued that the SSC would provide similar capabilities even though at a later time.

d) The interaction of CBA with the SSC was one of the more important questions discussed. Arguments were made that CBA will aid in training the manpower necessary to launch the SSC and that it will allow us to develop high luminosity detectors that can be used later on for the SSC. Some subpanel members also felt that CBA will be an important testbed to try out some of the accelerator physics ideas necessary for the SSC.

On the other hand, the majority of the subpanel felt that the accelerator physics questions can be investigated at the Tevatron, the detectors can be tested sufficiently in the Tevatron environment and the fixed target beams, and that the accelerator manpower pool is too limited already, so that construction of CBA would have to delay the SSC.

e) The question of physicist manpower was discussed at length by the subpanel. The subpanel did try to estimate as well as it could what the different high energy groups will be doing during the rest of this decade. The majority felt that the commitments of most of the groups were such that they would not seriously participate in the CBA work before the end of this decade.

The subpanel worried seriously about the health of the field and the opportunities available in the early 1990's were CBA not to be built. It was realized that the opportunities to do forefront physics in this country will be running out by that time; on the other hand, one cannot disregard the great deal of effort that it would take to develop and construct the necessary instrumentation for the experiments at the SSC.

f) Uncertainties connected with the SSC project. The subpanel was well aware about the many inherent risks connected with the SSC project, ranging from the lack of any certainty as to the federal funding to many questions about its time scale and the ultimate cost. Thus the subpanel considered the need for the CBA in the framework of both the best and the worst-case scenario for the SSC. The eventual majority decision was reached after considering all these implications.

g) Interaction with other programs. The subpanel considered the interaction of CBA construction with the other high energy programs around the country. It was clear that this interaction would be quite strong with the AGS program at Brookhaven, which would have to be seriously curtailed to achieved the CBA construction goals. There would also probably be some negative impact on programs at other laboratories. Even though these effects might not be major, they did enter into the consideration of the subpanel in arriving at the final recommendation.

h) Timescale of CBA and of the other machines under construction. In assesing the physics impact of CBA, the subpanel tried to take a careful look at the projected time scales of other accelerators presently under construction in this country. The subpanel tried to assess as well as it could the realism of these completion projections, both from the point of view of money as well as time.

i) Consistency with the previous Woods Hole panels. The subpanel did look at the recommendations of the previous Woods Hole panels on the CBA issue. At the same time, the subpanel did try to take into account some of the physics and technical developments that have taken place in the last two years that might cast a different light on some of these previous recommendations.

j) Unity of the High Energy Community. The subpanel appreciated that the SSC project is of such a magnitude that it can only succeed if the whole high energy community is behind it. Thus the subpanel was reluctant to put forth any recommendation that would alienate a significant fraction of the community. Its negative recommendation on CBA was made with full awareness that there will be many high energy physicists displeased with this decision.

I have tried above merely to outline some of the main issues that were discussed in connection with the CBA questions. I have not attempted to give all the arguments on each issue and how they would affect the CBA because I probably could not do full justice to both sides on each point. I hope that this outline will demonstrate that the question was discussed in depth and all possible nuances were aired in some detail.

I would like to end by trying to summarize as well as I can the majority argument for not approving the CBA project. Firstly, the majority felt that in light of the recent discoveries and the other accelerators coming on line in the near future, the physics productivity of the CBA will not be as high as originally anticipated. Secondly, there is a real danger that the construction of the CBA will interfere with the SSC project, which should be our top priority. And finally, it was felt that the CBA project is not absolutely essential for the vitality and health of the US high energy program either now or in the near future.

Mr. FUQUA. Thank you very much and we will now listen to Dr. Sandweiss.

STATEMENT OF DR. JACK SANDWEISS, DEPARTMENT OF PHYSICS, YALE UNIVERSITY, AND CHAIRMAN, U.S. DEPARTMENT OF ENERGY, HIGH ENERGY PHYSICS ADVISORY PANEL [HEPAP]

Dr. SANDWEISS. Mr. Chairman, thank you for the opportunity to appear before you and the committee to discuss the recent recommendations of the High Energy Physics Advisory Panel concerning new facilities for the U.S. program.

Dr. Wojcicki has reviewed the recommendations of the 1983 subpanel and the analysis upon which they were based. As you know, HEPAP has unanimously endorsed these recommendations. I would like to comment now on the reasons why this really very difficult, even painful action that we took was the result of the most thoughtful, I think, and responsible process, and was carried out with the clear view of the opportunities and the risks which it entails.

The panel was convinced that the importance to the progress of physics of proceeding to the high energy region was so great that it would have been unwise to take the risk of using the major resources, the funding and scientific manpower required to construct and to utilize the lower energy CBA, even though it would have been a useful and viable accelerator.

This conviction had three primary sources. First, as Dr. Wojcicki has explained, the great advances that have been recently made in high energy physics point to the energy range of the proposed multi-TeV hadron collider as a region in which new fundamental insights into the nature of matter will almost certainly be gained.

The progress of our science at its most exciting frontier just drives universal enthusiasm for the new accelerator among high energy physicists.

Second, we believe that the progress which has been made in superconducting technology in the last decade in the United States, at Fermilab and Brookhaven, particularly, makes such a high-energy, high-luminosity hadron collider a technically sound goal.

Finally, we believe that the multi-TeV collider is necessary to insure the competitive position of the U.S. high energy physics on an international scene. The panel was and is concerned about the health and vigor of the U.S. program during the period before the new accelerator could become available.

Vigorous support for the second recommendation of the subpanel—I don't think was mentioned today—which encouraged rapid completion of the current construction projects at Tevatron and Fermilab and the SLC linear collider, and thorough utilization of existing facilities will help.

Certainly, vigorous prosecution of the new proposed program leading to the collider would also help. Nevertheless, we realize that there may well be problems arising from the long period without a major new facility, a delay which is unavoidable, even if the new proposed accelerator would proceed on a fast track.

However, we believe the risk to the health of the U.S. program, had we done otherwise, would have been greater, and in a certain sense, it would have been a more fundamental risk.

I would like to make one last comment on the HEPAP endorsement. The recommendation of the subpanel with respect to the CBA was by a majority vote, but the endorsement of the recommendation of HEPAP was unanimous. This is, I believe, a reflection of the widely held belief among high energy physicists that there must be widespread support across the field for any major new facility we propose.

The unanimous endorsement of the subpanel's recommendations is a manifestation of the universal support and enthusiasm for the proposed new multi-TeV collider.

I would like to turn briefly now to the subject of the research and development effort which has been suggested by HEPAP as the first phase of the multi-TeV hadron collider project. Now, if the goal were simply to proceed as rapidly as possible to the new accelerator, such a major R&D effort would not be required, since existing technology could, we believe, be used to produce a successful accelerator.

However, our past experience with superconducting technology and the basic intrinsic features of high energy accelerators themselves lead us to believe with a high degree of confidence that very substantial savings, many times the cost of the R&D, could well result from the type and scope of R&D effort we proposed.

Perhaps it would be—perhaps it should be more a D effort than an R effort, to be more accurate, but there is the opportunity, almost guaranteed, of very substantial savings.

The effort is a major one, lasting perhaps 3 to 4 years. One lesson that has been clearly learned from our experience with superconducting technology is that theoretical and engineering calculations, as vital and important as they are, cannot be sufficient. Substantial experimental testing and prototype magnet production are essential and are basic aspects of the most efficient and the most-effective approach to R&D in the superconducting realm.

It is perhaps worth noting as well that the technical and human resources are available now to start on this R&D. The magnet development team at Brookhaven, which has created, in the CBA magnet, what is probably the most advanced superconducting accelerator magnet in existence today, will clearly be an important element. The magnet production and testing facilities, as well as the cryogenic facilities developed for the CBA effort, will be essential in the new R&D program.

With the transition of the Fermilab superconducting Tevatron from construction to operation, scientists and engineers are becoming available for work on the new R&D effort, and needless to say, their enthusiasm for doing so is great.

I might also point out that as the only superconducting accelerator in the world, the Fermilab machine itself will be a valuable tool in the R&D for the new accelerator.

In addition, because of the widespread interest in the high energy physics community, other groups are forming with the hope and intent to contribute to the R&D program for the new collider.

In summary, I believe we stand at a crossroads in U.S. high energy physics. We've had to make difficult decisions, but we have chosen the path we wish to follow. We believe it will lead us to a new level of insight into the heart of matter and leave U.S. high energy physics to a position second to none.

Mr. Chairman, that concludes my testimony and I'd be pleased to respond to any questions.

[The prepared statement of Dr. Sandweiss follows:]

Summary of Testimony
of Jack Sandweiss
Office of Energy Research
Subcommittee on Energy Developmnet and Applications
of the
House Science and Technology Committee
October 19, 1983

The High Energy Physics Advisory Panel (HEPAP) has unanimously endorsed the recommendations of the 1983 Subpanel on New Facilities. The reasons for this difficult action center around the Panel's conviction that the proposed multi-TeV, high luminosity hadron collider is essential for the progress of forefront physics and for the continued competitive position of U.S. High Energy Physics on the international scene. The panel further believes that the technology is at an appropriate state for the initiation of the effort towards the new accelerator.

Although the CBA would have been a useful and viable accelerator, the panel felt that it would have been unwise to divert the major resources of funding and scientific manpower required to construct and utilize the CBA, given the importance of proceeding as rapidly as possible to the high energy region.

The first phase of the new accelerator which HEPAP has proposed is a major R and D effort aimed at realizing potentially great savings in cost. Manpower and technical resources are available now to begin this R and D phase.

Statement of Jack Sandweiss
Office of Energy Research
Department of Energy
Subcommittee on Energy Development and Applications
of the
House Science and Technology Committee
October 19, 1983

Thank you for the opportunity to appear before you and to discuss the recent recommendations of the High Energy Physics Advisory Panel (HEPAP) concerning new facilities for the United States program. Dr. Wojcicki has reviewed the recommendations of the 1983 Subpanel on New Facilities and the analysis upon which they are based. As you know, HEPAP has unanimously endorsed these recommendations. I would like to comment on the reasons why this difficult, even painful, action was the result of a most thoughtful and responsible process, carried out with a clear view of the opportunities and risks which it entails.

The panel was convinced that the importance to the progress of physics of proceeding to the high energy region was so great that it would have been unwise to take the risk of using the major resources, of funding and scientific manpower, required to construct and utilize the lower energy CBA, even though it would have been a useful and viable accelerator.

This conviction had three primary sources. First, as Dr. Wojcicki has explained, the great advances which have recently been made in High Energy Physics point to the energy range of the proposed multi-TeV hadron collider as a region in which new, fundamental insights into the nature of matter will almost centainly be gained. The progress of our science at its most exciting frontier drives the universal enthusiasm for the new accelerator among high energy physicists. Secondly, we believe that the progress which has been made in superconducting accelerator technology in the last decade in the U.S.

-- at Fermilab and Brookhaven, particularly -- make such a high energy, high luminosity, hadron collider a technically sound goal. Finally, we believe that the multi-Tev hadron collider is necessary to insure the competitive position of United States High Energy Physics on the international scene.

The panel was, and is, concerned about the health and vigor of the U.S. program during the period before the new accelerator could become available. Vigorous support for recommendation 2 of the Subpanel, encouraging rapid completion of the current construction projects, the TEVATRON at Fermilab and the SLAC Linear Collider (SLC) and thorough utilization of existing facilities will help. Certainly, vigorous prosecution of the new proposed collider would help. Nevertheless, we realize that there may well be problems arising from the long period without a major new facility, a delay which is unavoidable, even if the new, proposed, accelerator proceeds on a fast track. However, we believe that the risk to the health of the U.S. program, had we done otherwise, would have been greater and in a sense more fundamental.

I would like to make one last comment on the HEPAP endorsement. The recommendation of the Subpanel with respect to the CBA was by a majority vote but the endorsement of the recommendation by HEPAP was unanimous. This is, I believe, a reflection of the widely held belief among high energy physicists that there must be widespread support across the field for any major new facility we propose. The unanimous endorsement of the Subpanel's recommendations is a manifestation of the universal support and enthusiasm for the proposed new multi-TeV collider.

I would like now to turn briefly to the subject of the Research and Development effort which has been suggested by HEPAP as the first phase of the multi-TeV hadron collider project. If the goal were simply to proceed as rapidly as possible to the new accelerator such a major R and D effort would not be required since existing technology could, we believe, be used to produce a successful accelerator. However, our past experience with superconducting technology and the basic features of high energy accelerators themselves lead us to believe with a high degree of confidence that very substantial savings, many times the cost of the R and D, could well result from the type and scope of R and D effort we have proposed.

The effort is a major one, lasting perhaps 3 to 4 years. One lesson that has been clearly learned from our experience with superconducting technolgy is that theoretical and engineering calculations, as vital and important as they are, cannot be sufficient. Substantial experimental testing and prototype magnet production are essential and are basic aspects of the most efficient and cost effective approach to R and D in the superconducting realm.

It is perhaps worth noting as well that the technical and human resources are available now to start on this R and D. The magnet development team at Brookhaven which has created, in the CBA magnet, what is probably the most advanced superconducting accelerator magnet extant today, will clearly be an important element. The magnet production and testing facilities as well as the cryogenic facilities developed for the CBA effort will be essential in the new R and D program. With the transition of the Fermilab superconducting TEVATRON from construction to operation, scientists and engineers are becoming available for work on the new R and D effort and needless to say their enthusiasm for doing so is great. As the only superconducting accelerator in the world, the Fermilab machine will itself be an invaluable tool in the R and D for the new accelerator. In addition, because of the widespread interest in the high energy physics community, other groups are forming with the hope and intent to contribute to the R and D program for the new high energy hadron collider. In summary, I believe we stand at a cross roads in U.S. High Energy Physics. We have had to make difficult decisions but we have chosen the path we wish to follow -- we believe it will lead us to a new level of insight into the heart of matter and lead U.S. High Energy Physics to a position second to none.

Mr. Chairman, that concludes my testimony. I would be pleased to respond to any questions you might have.

Mr. **Fuqua**. Thank you very much. I think at this time, it would be appropriate to hear from Congressman Carney who may have a different conclusion.

Mr. **Carney**. Thank you, Mr. Chairman, and I——

Mr. **Fuqua**. It's been rumored to that effect.

Mr. **Carney**. I apologize for going somewhat out of order, but I appreciate your consideration and I would like at this time to read my statement.

I believe there are two issues that must be addressed by the Congress as far as high energy physics is concerned, and the first issue would be, do the recommendations contained in the report advance not only the interests of the scientific community, but also the American taxpayers?

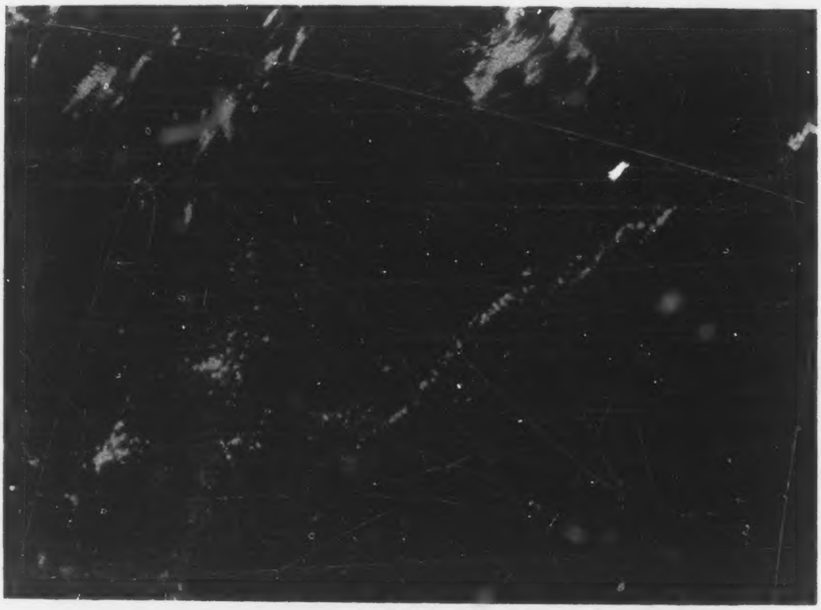
The second issue is, what responsibility does the scientific community have to Congress for the development of a consistent scientific policy? The fragile and unique relationship between the scientific community and the Congress requires an examination of both these issues.

In regards to the first issue, the report of the subpanel on new facilities recommended the termination of the construction of the colliding beam accelerator at Brookhaven National Laboratory. And as my colleagues on the subcommittee are aware, the Federal Government has invested over $200 million in the research, development, and construction of the colliding beam accelerator.

I have distributed to some of my colleagues, and I'd be glad to show—well, you'll have an opportunity to see them, gentlemen, because we have a slide presentation—a booklet describing the current project status of the CBA.

I would like to briefly highlight the following major milestones that have already been achieved at the CBA for those people who are here. [Slide 1.]

Slide 1



Aerial View, as seen from Landsat 4, of Metropolitan New York and Long Island. The CBA Tunnel is clearly discernible.

I don't know if you can clearly see with the light, but that's the picture of Long Island from Landsat and the only physical characteristic that you can see from our Landsat satellite photograph is, in fact, the colliding beam accelerator at Brookhaven. I just bring that point out to show the size and the magnitude of the construction project that's going on at Brookhaven National Laboratory.

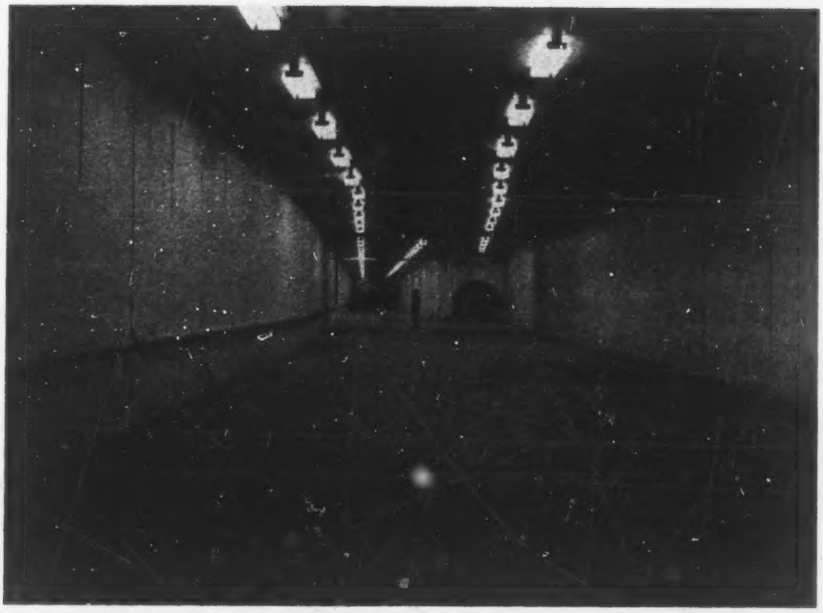
It doesn't pick up the Empire State Building, the World Trade Center; it doesn't pick up anything; it picks up the colliding beam accelerator. [Slide 2.]

SLIDE 2

Aerial view of BNL, including the CBA Site.

The next picture we have is the aerial view of the Brookhaven National Laboratory and it includes behind it that large circular ring which is the colliding beam accelerator site. It's 2.4 miles in diameter. [Slide 3.]

Slide 3



The tunnel to the right would bring the beam from the existing AGS. The Wide-Angle Experimental Hall at 6 O'clock is seen in the distance on the left.

The third picture, if you would, Eric, is a picture of the tunnel inside. We're seeing where it interfaces with the AGS. That's the beam coming in from the AGS, going into the wide-angle experiment hall at 6 o'clock, as seen in the distant left. The tunnel on the righthand side is the connecting tunnel with the AGS. [Slide 4.]

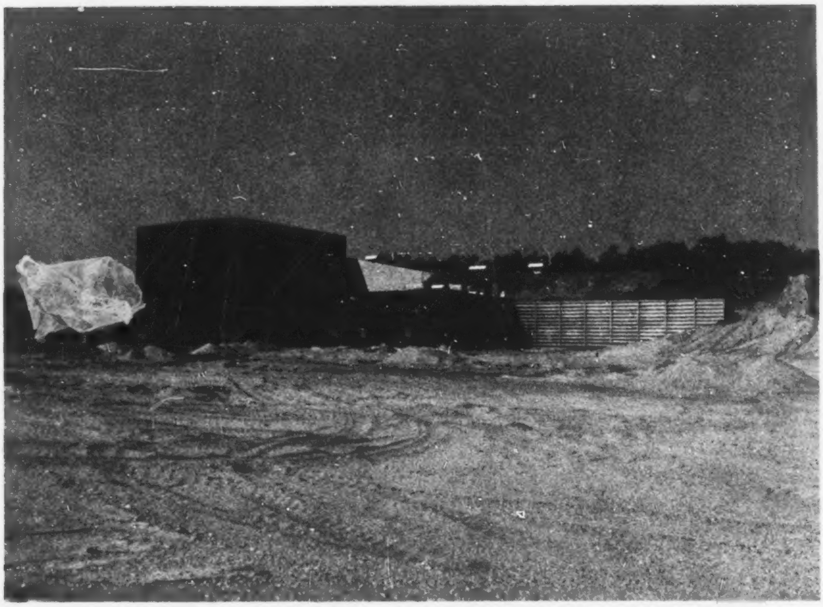
SLIDE 4

The Central Service Facility, as seen from inside the CBA Ring. From left to right: Main Control Building, Cryogenic Wing, Compressor Structure, and Cooling Towers.

We move along now and we'll see that the central service facility is complete, and in that is included the control room, the office laboratory and space. This picture will bring this out. There are other parts you see that are not part of the central control room, but as long as the slide picture is there, we'll describe that.

We have the cryogenic wing, the compressor structure and the cooling towers. The cooling towers would be—I know it's a little difficult to see in the black-and-white picture—that building on your righthand side. In the middle, you can see the cryogenic wing and the helium tanks, et cetera, and as we said before, the central service facility. [Slide 5.]

SLIDE 5



Exterior view of the Major Experimental Facility at 8 O'clock — One of four completed research areas.

As we move along, we will see an exterior view of the major experimental facility which is located at 8 o'clock. It's one of the four completed facilities. A decision was made not to build the other two, so in essence, the facilities have already been completed and constructed, as you can see. [Slide 6.]

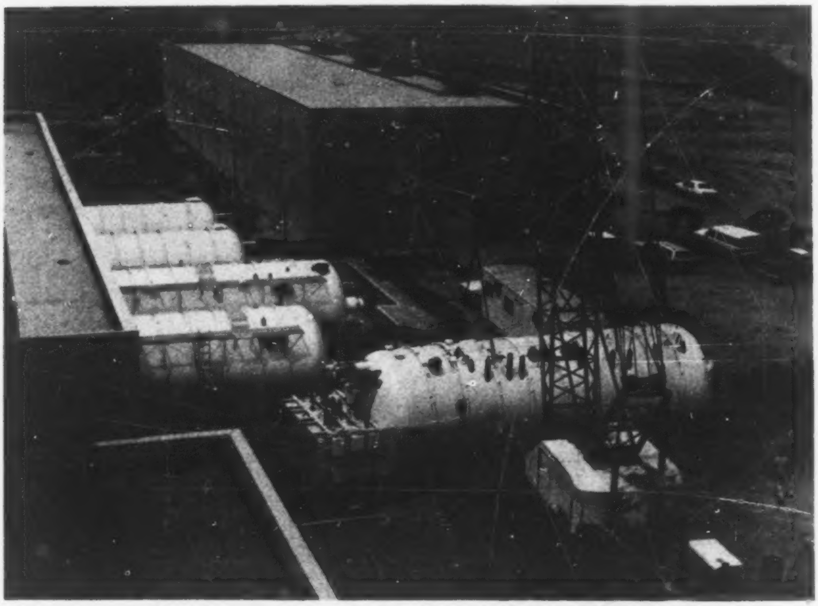
SLIDE 6

5. Interior view of the 8 O'clock Major Facility. The machine tunnel continues in the distance.

We can move along and now we look at the refrigerator and cold-boxes. They're delivered—I'm sorry—we're still on the interior. You can put that back up, Eric. Thank you.

We have the interior view of the major facility. It's hard to understand the depth, but we're talking about an area there that's, I believe, 15 by 19 meters, and we see a larger part of the tunnel which comes into the facility area. You can see it exiting and then in the depth of the picture, you can see the smaller part of the tunnel going around, so that just gives you an inside look of the facility. [Slide 7.]

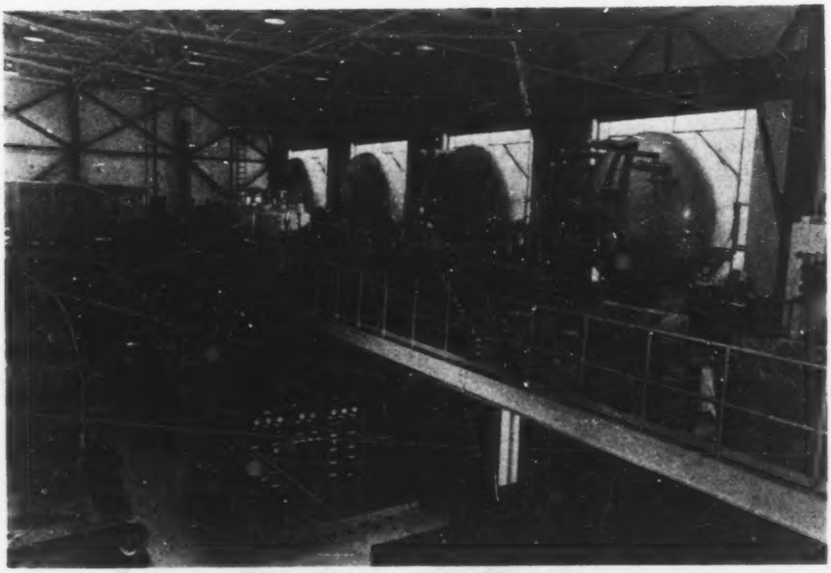
SLIDE 7



The World's largest helium refrigerator, designed to cool all of the superconducting magnets in the tunnel to near absolute zero. The last of five "cold boxes" is here being installed. Connections to the remaining equipment are made inside the building to the left. The building, in the center, houses the helium compressors which supply the refrigerator.

Now, we'll move along to a very important segment of the whole project, and that's the world's largest helium refrigerator. It's designed to cool all of the superconducting magnets in the tunnel to near absolute zero. You can see they're installing the last of five cold-boxes here. It's already done and the connections to the remaining equipment are made inside of the building to the left. The building is the center. It houses the helium compressors which supply this refrigeration. [Slide 8.]

SLIDE 8



This is the interior of the refrigerator building. The inner ends of the cold boxes can be seen protruding through the wall. (The fifth cold box is just out of the picture to the right.) The remaining equipment comprising the refrigerator can be seen on the mezzanine and on the floor below.

Now we go inside the building and you can see the interior of the refrigerator building. The inner ends of the cold-boxes can be seen protruding through the wall. The fifth cold-box is out of the picture, but it is there now.

The remaining equipment, comprising the refrigerator, can be seen on the mezzanine and on the floor below. So I think this adequately demonstrates how far we've advanced.

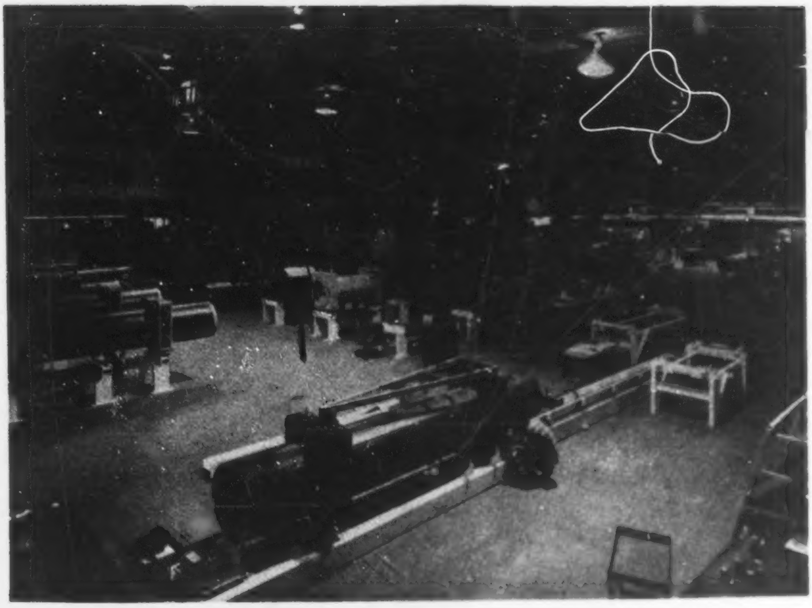
If we now go on——[Slide 9.]

Slide 9

One of the superconducting magnets produced for CBA, undergoing preliminary tests. The production models of these magnets have met all design requirements.

We can look at another segment of this. This perhaps is not part of the physical construction of CBA, but it's very, very important to understand how much progress has been made. This is one of the superconducting magnets produced for the CBA, undergoing preliminary tests. The production models of these magnets have met all the design requirements so we'll move from that slide to a——[Slide 10.]

SLIDE 10

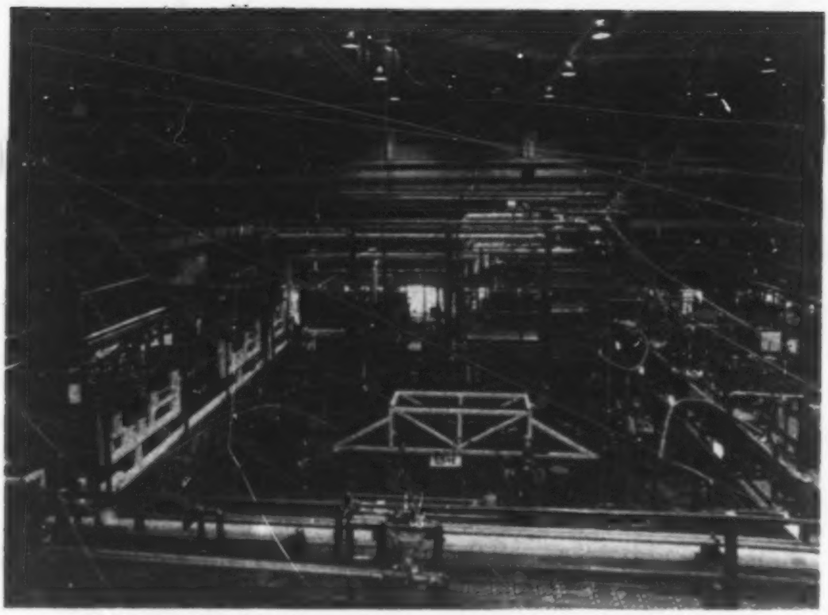


A small portion of the factory space set up to produce the superconducting magnets for CBA.

Small portion of the factory space that's already set up to produce the superconducting magnets for the CBA. So that facility is completed. It's tooled and ready for manufacturing, which is very important.

If we could go to the next slide, Eric. [Slide 11.]

SLIDE 11



This is the facility for production testing of the superconducting magnets under actual operating conditions. The system is fully operational, and a magnet can be seen mounted at one of the five test positions in the center.

We'll see that the facility—this is the facility for the production testing of the superconducting magnets under actual operating conditions. The system is fully operational and magnets could be seen mounted at one of the five test positions in the center. So we're ready to go on that. [Slide 12.]

SLIDE 12

A group of eight superconducting magnets mounted in the tunnel for a system test. This test was highly successful, showing that installation techniques, vacuum systems, cryogenics, magnet performance, and quench protection all worked as planned. This system was cycled to full CBA operating magnetic field 3,000 times without difficulty.

Finally, in the last slide, we'll see a group of eight superconducting magnets mounted in the tunnel for a system test. The test was highly successful, I might add, showing that the installation techniques, vacuum systems, cryogenics, the magnets' performance and quench protection all work as planned. The system was cycled to full CBA-operating magnetic fields 3,000 times without any difficulty.

The point that we're trying to impress people with right now is the fact that, indeed, the CBA is well on its way and many of the problems that we had have been solved and much of the construction is completed. In fact, from the standpoint of bricks and mortar, I'd have to say that 90 percent of it has been completed. From the standpoint of being capable of putting the magnets in, I think we've amply demonstrated it's well on its way.

The utilization of this facility and the recovery of the Federal Government's investment in this facility is not addressed in the report. Instead, the scientific community is proposing the abandonment of the CBA facility at this point and to begin the immediate initiation of the design and construction of a new proton-proton colliding beam facility, which is estimated in the report to be about 30 kilometers in diameter, and which could cost the taxpayers over $2 billion when completed in mid-1990's.

And frankly, I think the figures considered within the high energy physics community, may be somewhat conservative. I think they are somewhat low as far as both the time and the cost to construct a new facility.

I have serious concerns about the abandonment of the Federal taxpayers' commitment and the investment that they made in favor of this new dream machine. I have no problem with building the SSC. My problem comes with leapfrogging the CBA. Apparently the Secretary of Energy also shares my concerns over the abandonment of the half completed facilities, because when he was asked about the Clinch River project, whether that should be scrapped, he replied, and I quote:

> I've made the point that under our new philosophy, if we were starting today, we would not propose a Clinch River project to be funded federally, but, having gone as far as we have, I think we have to complete it. I don't like this business of stopping things in the middle. I think it's a waste of money.

That's the Secretary of Energy's quote.

And finally, in regard to the second issue, as Dr. Trivelpiece candidly notes in his testimony, HEPAP has reviewed the CBA in 1974, 1975, 1977, 1980, and 1981 as well as the latest review.

Prior review panels have recommended the funding of the research, development and construction of CBA and the Congress has acted in reliance. We've gone along with what you say. We go out, spend $200 million of taxpayers' money because you asked us to—you suggested that we do just that.

That places into question the relationship between the scientific community and Congress. It is essential, therefore, to the continuation of these relationship that a consistent scientific policy be developed and that the scientific community appreciate the economic commitments that have been made by the American taxpayer through us, the Congress.

They have to appreciate the position that we're now in. And I think that these are the two questions that, quite frankly, have not been adequately addressed with the Woods Hole Panel.

In conclusion, I might note that both Dr. Wojcicki and Mr. Sandweiss stressed rather strongly in their statements that the whole community must be behind it. And, indeed, the full panel on HEPAP recommended unanimously the discontinuation of Brookhaven's CBA. But let's be honest, the subpanel was somewhat divided on that recommendation. If I'm not certain, I believe with a certain amount of truth the vote was even closer. It was published to be 10 to 7 in favor of the recommendations but indeed it was 9 to 8, one changed to try to spread it apart.

So this was not a clear consensus of the subpanel. And that's the subpanel that did the complete study; the group that went from laboratory to laboratory to try to make the determination.

So I appreciate the chairman allowing me to make this presentation, bring up the two points that I think are very, very vital. And I will yield back to the Chair because I'm sure the chairman would like to question our distinguished panelists at this point.

Mr. **Fuqua**. Thank you, Mr. Carney.

Dr. Sandweiss, it is my understanding that the final vote was 10 to 7 in the subpanel, and maybe I should direct that to Dr. Wojcicki.

Dr. **Wojcicki**. I think that's correct, yes.

Mr. **Fuqua**. What, Dr. Sandweiss, would have been your opinion had the vote been reversed 10 to 7 in favor of the CBA?

Dr. **Sandweiss**. Well, one thing I've learned is it is very difficult to predict the actions of all of my colleagues.

Mr. **Fuqua**. I am familiar with that.

Dr. **Sandweiss**. Right. And so I have to say I really don't know what would happen. I believe—I—many of the things that Congressman Carney mentioned are very much on my mind, and I take them very seriously, and we have taken them very seriously.

One of the most serious problems——

Mr. **Fuqua**. Are all of these matters considered that Congressman Carney——

Dr. **Sandweiss**. I believe so. I mean——

Mr. **Fuqua** [continuing]. Mentioned in your deliberations?

Dr. **Sandweiss** [continuing]. It was certainly our desire and intention. And, in fact, I mentioned somewhere in my testimony that this was, in fact, a painful as well as a difficult decision. And I would really like to emphasize that. We tried to do the best by the taxpayers' money and efforts. And sometimes that requires recognizing that situations have changed; that it's, in a way it's very sad that they have changed. It's—but we have to be honest and call the shots as we honestly see them.

However, to return to your question. I think there would have still—still would have been very serious concern about the CBA because if approximately half of the high energy physics community was not in favor of proceeding, then I can't say how it would have come out. It might still have come out negative in the panel. I don't—I honestly don't really know. It might not have because of the fact that—that the amount of resource needed, not just to complete the CBA, but to utilize it properly, and certainly one would not wish to complete and not utilize it properly, is very considerable. And it was the—pretty much the universal view that that would have had an impact on the initiation and the top possible time scale of the new machine.

But I'm afraid that's the best I can do in answering your question. I really don't know.

Mr. **Fuqua**. Has there been any occasion that you recall when the HEPAP panel rejected the recommendation of a subpanel?

Dr. **Sandweiss**. I don't recall it because I've only been chairman for about 1½ years. I had been a member earlier in the seventies, and at that time that had not happened. So I haven't made an exhaustive study but I don't believe it has happened.

Mr. **Fuqua**. You don't—you do not——

Dr. **Sandweiss**. I don't believe so.

Mr. **Fuqua** [continuing]. Recall any time?

Dr. **Sandweiss**. I haven't really—certainly nothing major. This is really an unfortunate circumstance in that, first of all, you hope it doesn't happen. I'm sensitive to the inconsistency and it's bad.

Mr. **Fuqua**. When you and Dr. Wojcicki presented the SSC concept to the National Science Board, what were their concerns of the concept?

Dr. **Sandweiss**. Well, maybe I could comment briefly and Dr. Wojcicki could comment about this.

Mr. **Fuqua**. We would welcome that.

Dr. **Sandweiss**. I thought that they were—I thought they were really fairly supportive of the idea. They were very interested in why this was, at this stage, a necessary step for us to take. They were very interested in the excellence of the U.S. program in high energy physics and the importance of it remaining something which is second to none internationally.

I don't remember—there were—of course, there were some questions, of course, about the—why had the physics picture changed? Why is it that a good panel in 1980 would recommend going ahead but a—this panel, and HEPAP, recommended not going ahead; and we attempted to explain those much in the way that Stan and I have done. Maybe Stanley could comment further.

Mr. **Fuqua**. In the subpanel's report, they recommended that the DOE high energy physics program would probably increase substantially, some $750 to $800 million a year. And you indicated the—well, I think it was Dr. Wojcicki—the importance and the politics of getting support from the scientific community to support that.

Have you had assurances that you will get support from people such as material scientists or chemists?

Dr. **Sandweiss**. We certainly can't have assurances. We haven't had assurances. I think the spirit really is one of recognizing the risks but weighing the risks. I mean, there's the risk in the other action as well, and there's no course without risk for us, we realize it.

But we hope that we can—we hope that we can convince them that this is something which is scientifically worth it. And I should say in our letter of transmittal we did say that certainly our view was not that this was more important than other sciences but that we hoped that it would be possible, so to speak, to do this as an example, or part of an example of improving support for basic science as a whole.

It wasn't that we were saying this is more important than biology, or something like that.

Mr. **Fuqua**. Dr. Trivelpiece, it was suggested that at Brookhaven there be heavy ion research work conducted.

Would that enhance or interfere with any of the high energy physics activities currently going on at Brookhaven?

Dr. **Trivelpiece**. I don't believe so. I believe it would enhance them. You said both "enhance or interfere with." I think it would enhance them in the sense that it would provide a heavy ion capability for research at Brookhaven that does not now exist, and takes advantage of preexisting facilities.

There has been a study by the Nuclear Science Advisory Committee, which is another one of the Department's advisory committees—we share that with the National Science Foundation—that indicates the relativistic heavy ion collisions, in fact, offer an unusual opportunity for a rich area of science.

However, some preliminary work that needs to be done in order to verify that the predictions of the theoreticians and the estimates of the panel are in fact realized; and that's what we are proposing to begin as an initial step which could at some stage lead to utilization of the ISABELLE tunnel for nuclear physics as distinct from high energy physics.

Mr. **Fuqua**. Well, who in the scientific community is wanting to do this type of research? Is it the high energy physics or the nuclear physics group?

Dr. **Trivelpiece**. There has been a rather interesting reconvergence of interests of nuclear physicists and high energy physicists over the past few years. But the kind of picture that has emerged as quark gluon plasmas. The picture of the substructure of atoms, shows that the distinctions between high energy physics and nuclear physics are a little bit more blurred. Some of the things that would result in the collision, the relativistic collision, between two heavy ions could create the kind of quark gluon plasma that would reveal information that would also be of interest to high energy physicists. And, likewise, some of the things that high energy physicists are currently doing have become of rather intense interest to nuclear physicists.

So the dividing line which, although originally somewhat artificial and created for budgetary purposes has now, for scientific reasons, become a little bit more blurred and perhaps Dr. Sandweiss or Dr. Wojcicki would like to comment on that aspect of it.

Dr. **Sandweiss**. I would be happy to. It's in a way, what it really represents is that in a sense as the most basic frontier advances, the new areas of science get created. One of the nicest examples, to my mind, is the—when, of course, quantum mechanics became developed in the late twenties and early thirties, it could immediately be applied to many systems such as solid state physics. And the whole area, the whole solid state electronics area of science just grew out of the application of quantum mechanics and became quite another—another, but very large area in physics. In fact, solid state physics is the largest division in the American Physical Society and, if you like, that grew out of the entering wedge.

In a certain sense the discoveries in elementary particle physics about the character of quarks and gluons has led to a prediction that there should be a new form of matter, this so-called quark gluon plasma. People still debate some about its properties and so forth, but in a way if that form of matter is to be explored, it will represent a kind of new area of science—somewhat analogous to the application of quantum mechanics to solids which led to condensed matter physics, which in a way is—was an area that didn't exist in 1920, but now it does exist.

So, the people who will feed into this will be people from what are currently known as the nuclear physics community and some also from the high energy physics community.

Mr. **Fuqua**. Dr. Trivelpiece, are there any alternatives to the SSC, for instance, are there any facilities in the United States that can be expanded or upgraded, or can we participate in maybe such facilities as the one at CERN that can be modified at less cost and less time?

Dr. **Trivelpiece**. Well, as the other two witnesses have said——

Mr. **Fuqua**. Fermi or someplace?.

Dr. **Trivelpiece** [continuing]. This morning, the need is to have a mass of about 1 TeV particle created in order to make the proper explorations. Now, the exact number is not known. The CERN facility, by virtue of the size of its tunnel, could, if magnets were put in at the present limit of technology and perhaps maybe not even capable of being done yet, achieve about a 10 TeV collider. That may not——

Mr. **Fuqua**. The LEAP facility?

Dr. **Trivelpiece**. Pardon?

Mr. **Fuqua**. The LEAP facility?

Dr. **Trivelpiece**. At CERN——

Mr. **Fuqua**. LEP, I mean.

Dr. **Trivelpiece**. L-E-P.

Mr. **Fuqua**. LEP, I'm sorry.

Dr. **Trivelpiece**. I thought you were talking about leapfrogging. [Laughter.]

Dr. **Trivelpiece**. The LEP facility, with 10 tesla magnets, could provide a collider at approximately 10 TeV. However, I think more realistic, given the nature of the tunnel and so on, it is likely that 5 TeV would be about the energy range they would be planning for achieving as Dr. Wojcicki indicated.

The facilities in the United States are not, at the moment, in a position to go to 20 TeV. In fact, nobody is, there is just a requirement for size of a—the accelerator ring at the magnet technology that could be built at this stage is just very large.

That was the point I think Dr. Sandweiss was trying to make. There is not as much research involved in this as there is development. Basically all the things that you need to know how to do are known at the moment. It is merely a question of trying to develop a design which would be at a low enough cost that it could be realistically expected to be built.

We could certainly take advantage of cooperation with CERN. Just last week or the week before last, as the representative for the United States, and the United States has the lead in exploring international cooperation in high energy physics under the terms of the DESY summit agreement and Williamsburg agreement. And we did explore various possibilities.

It turns out that the European CERN facility has more or less made a commitment to build the LEP tunnel and do electron/positron research in the LEP tunnel and then should it prove to be desirable, consider going forward with a proton-proton collider in that same facility.

However, we don't quite know what their plans are going to be precisely but they certainly are interested in the possibilities of cooperation and as are we, should that be a cost-effective way to proceed most expeditiously with the science.

Mr. **Fuqua**. Well, are there any alternatives such as involving international cooperation or cosponsoring by a variety of nations?

Dr. **Trivelpiece**. Yes; I think that possibility certainly exists. And as a result of that DESY meeting, in a report which we will deliver to the heads of State in the London summit next year, we are beginning to look into ways by which we might engage in international cooperation with these kinds of facilities that has not been done in the past.

I think there are several areas of science now in which the frontier is beginning to involve considerations of facilities that may well exceed a billion dollars in cost. And it would seem prudent and advantageous if it were possible to have these facilities planned and developed on an international basis in a way that didn't require each nation to simultaneously build precisely the same object. It makes just good commonsense.

However, there are matters of national concern that come into play and the whole problem of whether or not we can build real property on other national sites, or whether we can transfer real property, or how we would transfer funds and so on. There are some very interesting legal international issues associated with this which would have to be resolved in advance of any kind of serious planning toward such a program.

But we are beginning those steps to explore how we can go about looking into that kind of international cooperation, and it is somewhat unprecedented in the size, as I understand it.

Mr. **Fuqua**. Well, let me encourage you, because I think when we are talking the numbers that we are about the cost, plus the operational cost, that it certainly behooves us to seek as much participation as we can to share some of the burden as well as the results of a program.

Mr. Carney?

Mr. **Carney**. Thank you, Mr. Chairman.

Dr. Sandweiss, you said before to questioning of the chairman that we are talking about $750 or $800 million a year for high energy physics.

Dr. **Sandweiss**. No, he said that. I didn't say that.

Mr. **Carney**. Is that what we would be talking about if we went with the superconducting super collider SSC?

Dr. **Sandweiss**. What we have proposed at the present time is to start this R&D program. That program has, as I indicated, as its primary goal lowering the costs and also of discovering what the cost of the machine actually would be.

I would say that it's—my view is that at this moment I wouldn't want to venture what it would cost until we see the results.

We have some very good ideas, we feel, about utilizing the past technology and utilizing the high energy character of the beams themselves, the smallness in size that comes with going to high energy, for example, to make the cost smaller, and there are good possibilities there.

I guess what I would say is we should carry out the research. I would propose that we vigorously do that and see where we end up before—I mean, that will certainly happen in any case; we wouldn't go ahead without having research.

So I wouldn't venture a number because I don't know the outcome of that research, although I am very optimistic that it will be——

Mr. **Fuqua**. Will the gentleman yield?

Mr. **Carney**. Yes, sir, I would be glad to yield.

Mr. **Fuqua**. It was understanding that was the subpanel's report.

Dr. **Sandweiss**. Yes, he asked me would I——

Mr. **Fuqua**. Yes, yes.

Well, what type figure are we talking about?

Dr. **Sandweiss**. I think perhaps the budget for next year, for 1984, is, I believe, as Dr. Trivelpiece indicated, certainly one could use somewhat more than that. But it's—it's of that general order. I don't know the exact numbers for the following years—there will be other demands and so on.

Mr. **Carney**. Would you repeat that number again, the gen—of that general order, what was the number?

Dr. **Sandweiss**. Whatever it is for this coming year. Perhaps Dr. Trivelpiece can say what it is for 1984.

Mr. **Carney**. Dr. Trivelpiece, could you answer that question?

Dr. **Trivelpiece**. The high physics budget for 1984 is approximately $500 million.

Mr. **Carney**. Dr. Sandweiss, do you know what the number was for the Deutch ceiling?

Dr. **Sandweiss**. Ah, let's see, I get mixed up in which year's dollars it is. I don't know what it is in current-year dollars but I—I recall that it was probably something a little greater or right around what we—around the number we now have. But I am not really sure of——

Mr. **Carney**. Four hundred and——

Dr. **Sandweiss** [continuing]. What is back of it?

Mr. **Carney**. Four hundred and forty million in 1982 dollars was the Deutch ceiling.

Dr. **Sandweiss**. OK.

Mr. **Carney**. The reason I asked that question is that we agonize through many of these reports——

Dr. **Sandweiss**. Yes.

Mr. **Carney** [continuing]. And we will go to the Trilling report. The big agony of the Trilling report was should we continue to go with CBA or at that time ISABELLE, and the whole thought there was we were locked into the Deutch floor of funding for high energy physics and the conclusion was, yes, we could continue to go—we would have enough money to finish that.

Why, in 10 months time, does the community feel that there are so many more millions of dollars available where we would exceed the Deutch floor—why did they feel that that money would be available and we could go on to build the dream machine, where even in their recommendations they are talking about $500 million for each fiscal year for the next three fiscal years and between $750 to $800 million for several years after that during the construction phase of SSC?

Where were they led to believe this would be available?

Dr. **Sandweiss**. I don't think they—they were not led to believe that it would be available. What they did was to weigh risks.

Mr. **Carney**. Pardon?

Dr. **Sandweiss**. They weighed risks, risks of doing—of proposing it versus the risk of not proposing it. In other words, if we went ahead with the CBA, people saw many other risks to the community to high energy physics. And it was a—certainly, there—it's not free of risk, nobody thought that.

Mr. **Carney**. I appreciate that but what I am confused with was when we had Trilling 10 months before Woods Hole, we were concerned about the Deutch floor. Ten months later, we are talking about outyears of 800 millions of dollars for high energy physics.

Why did the community get stimulated, or what gave them the stimuli to think that there would be this great increase in high energy physics?

Why was it in 10 months we had a deal with a platform of money? Congress certainly hasn't said there would be more money. No one has. Why did they think that there was all this money to go forward with almost doubling of the amounts of money per fiscal year for the SSC?

Dr. **Sandweiss**. As I said, I don't—they didn't know that it would be forthcoming. What—there were many reasons why that was—that recommendation was made. It was—certainly there was a hope that the discoveries that had been made in physics that the climate, perhaps, toward basic research, partly as a result of those discoveries, and partly for other reasons, that it might be possible to take this step which they felt—we felt, was vital for our U.S. high energy physics program. Partly, I think, it was a realization that our country was sadly becoming second best, or maybe third best. That was very worrisome, and people thought there might be a response to that. But they certainly didn't know and they recognized that they were weighing two courses of action with risk on either side.

Mr. **Carney**. Thank you.

Dr. Wojcicki, did I pronounce that right?

Dr. **Wojcicki**. You have done as well as most people. [Laughter.]

Mr. **Carney**. Thank you.

Dr. **Wojcicki**. Thank you very much.

Mr. **Carney**. On page 1038 of the September 9 issue of Science magazine, it was noted that the President's Science Adviser, Dr. George Keyworth III, met with your subpanel last spring, in Washington, in fact. He told the subpanel that if a well thought-out proposal were put forward, one that had the backing of the entire high energy community, that he would support it.

Furthermore, given that the project would take a decade, a price tag of a few billion dollars might well be feasible. Is that true?

Dr. **Wojcicki**. I don't remember the—certainly I think the first statement, I think, is true, as to his personal support for a well thought-out project in high energy physics.

I do not—I think—I don't remember the—whether the price tag of many billions of dollars.

Mr. **Carney**. That is the quote attributed to Mr. Keyworth. I will read it again. The quote is: "That if a well thought-out proposal were put forward, one that had the backing of the entire high energy physics community," then he would support it. "Furthermore, given the project would take a decade, a price tag of a few billion dollars, might well be feasible."

Dr. **Wojcicki**. OK. Like I said, I'm not sure that I can recollect whether the few billion dollars was the number that was stated or whether it was phrased in a different manner. But I think the gist, if I remember correctly, of that comment, was that a well thought-out project with the whole community, high energy community, behind it, would receive his support even if it would need some increase in funding for the period of construction. And I think——

Mr. CARNEY. Is it unusual to have a person who holds the title that Dr. Keyworth holds meet with the subpanel and before they meet and kind of give them a direction, maybe?

Dr. WOJCICKI. Well, I am not sure that there was a direction and, again, Jack maybe, or Dr. Trivelpiece could comment more about the customary procedure. We have tried to meet with as many people as possible. I think we have met with relevant people from the Department of Energy, from the National Science Foundation, and we felt that, therefore, that—people from that office, you know, it would be useful for the subpanel to hear what the high-placed people in that office feel about the future of science in general in this country.

So I don't think that that was extremely unusual, but I may be wrong. Like I say, I don't know what the past history was.

Mr. CARNEY. Dr. Trivelpiece?

Dr. TRIVELPIECE. I generally have invited Dr. Keyworth to attend opening panel meetings in a number of circumstances such as this, including the Versailles-Williamsburg summit follow-on, and so on, where there are possible matters of policy that might come in in order to have him have an opportunity to express himself.

I don't remember the quote of $1 billion either. I certainly remember the first part. In fact, in my own statements to the panel I made essentially the same remarks and in that sense, what we believe their role to be is to tell us what the proper science is and to develop a plan—it goes almost without saying, that if they develop a bad plan, that obviously won't have our support.

So what we are asking them to do is tell us what the science is and to develop a reasonable plan. We can't guarantee that the funding is going to be available either, and try to put in the proper cautionary statement, but first tell us what the science is and then we will try to determine whether or not it is feasible within the kinds of budgetary constraints that the department has and, of course, to present this to you for discussion also.

Mr. CARNEY. Well, I can understand that, but what I am confused about is then why, after just a short period of 10 months, did you ask HEPAP to consider for a second time the subject matter on the earlier report made by the Trilling panel?

Dr. TRIVELPIECE. The Trilling panel did not address the issues as crisply as I had hoped they would. They, in fact, gave us a—what I call a graduated or an incremental answer—that is, do this if you have this much, do this much more if you have this much more, and so on.

What I was seeking was not a commentary in the area in which they are not experts, namely, what to do under certain budgetary constraints but, rather, I was asking them what is the most important science that this field should address? And that is the nature of the letter that I addressed to Dr. Sandweiss.

Mr. CARNEY. Well, in other words——

Dr. TRIVELPIECE. Part of my testimony.

Mr. CARNEY. Let me ask you a question. Was the Trilling committee unanimous in their findings?

Dr. TRIVELPIECE. I don't recall.

Mr. CARNEY. Dr. Wojcicki, would you know if the Trilling committee was unanimous?

Dr. **Wojcicki**. No; I certainly would not.

Mr. **Carney**. Dr. Sandweiss, would you know if they were unanimous?

Dr. **Sandweiss**. I don't recall, but I think they were—they were—if they were not unanimous, I think in the end they were close to unanimous.

Dr. **Trivelpiece**. It's the same thing with that subpanel as with this subpanel; that is, HEPAP is the only advisory committee that we can pay attention to and not the subpanels directly. So to that extent it was the HEPAP panel that took the Trilling report and delivered it to us with their recommendations, I believe that was it.

Mr. **Bateman**. Would the gentleman yield on that point?

Mr. **Carney**. I would be glad to yield to my colleague.

Mr. **Bateman**. I—I'm intrigued by that aspect of the layer of recommendation. The subpanel, in this instance, was divided at one point, 9 to 7, or 9 to 8, ultimately 10 to 7. The HEPAP was unanimous.

My experience in life would indicate to me that when one group is unanimous on a recommendation of experts in an area who are significantly divided, it generally bespeaks the absence of an independent in-depth critical analysis but the acceptance of a majority wisdom of the subpanel.

Is that the case here?

Dr. **Trivelpiece**. I think it would be more appropriate for him to answer.

Dr. **Sandweiss**. I would be pleased to answer that.

No, that's not the case here. First of all, many members of HEPAP—in fact, we are also members of the subpanel. There is a substantial overlap in membership.

Second, others of us—for example, I attended all the subpanel meetings and listened very closely to all—all of the meetings. Also, in addition to that, I would say many of the members of HEPAP had been wrestling with these issues for a long time.

What it really represented, I believe, is a recognition that we had to choose a course. And, as I said earlier, that no course was perfect, no course was free of risk. But it was clear to us that we had to choose a course. We had to finally decide what we were going to do. And the unanimity of the HEPAP vote was a reflection of that—that decision that—that we were all in favor, and very, very concerned about trying to achieve the high energy machine. And we were convinced that given, in fact, that split vote and a majority against the CBA, that we could not achieve that if we didn't go make our course and then go in that course.

So in a sense that's the meaning of our—our unanimous vote.

Mr. **Bateman**. If—if the gentleman——

Mr. **Carney**. If I may at this point, I would just like to say, Dr. Sandweiss, would you be saying, then, that on the scientific merit of the subpanel they were divided, but when it came to the full panel there was a recognition that you had to have the unanimity of the community? You had to go forward, you had to project this project out so it enjoyed the support of the total community? And could, therefore, hopefully, get the support of Congress, and move forward; project the U.S. high energy physics program in a preeminent position in the world?

So, therefore, other scientists said, we will give them that, let's get behind it and go; is that what we are saying?

Dr. **SANDWEISS**. It is essentially that in the sense that we—we all believe, even the people on the other side, that we had to make a choice in doing this.

Mr. **CARNEY**. A consensus?

Dr. **SANDWEISS**. That's why that we needed a consensus that we could all believe in. Now, if there were not a support, if people—if it were not for the universal agreement that the new machine was the direction that we wanted to move, then I don't think this would have happened.

So it is not a meaningless consensus by any means.

Mr. **CARNEY**. I am not saying that.

Dr. **SANDWEISS**. No.

Mr. **CARNEY**. I am not saying that.

Dr. **SANDWEISS**. Yes.

Mr. **CARNEY**. But what we are doing—you are saying that you needed a consensus to make—to project, you might want to say——

Dr. **SANDWEISS**. And to conserve our internal energies, as well.

Mr. **CARNEY** [continuing]. And to have a perception out there.

Dr. **SANDWEISS**. No, not just a perception. I mean, we—in order to have the internal energies of the field which has a limited manpower and limited human resources as well as potential financial resources. We needed to go in that direction. We were all convinced in the end that we needed to go in that direction.

The subpanel, in a way, weighed all the questions. But they were not in a sense faced with it, they were making recommendations to us. They were not making, if you like, recommendations as to how the field should finally act on these.

Mr. **CARNEY**. OK. The subpanel on the merits of the sciences divided on who have the majority of an opinion, I believe it was 9 to 8 and then 10 to 7. And then you move forward to the full panel—the full panel recognizing that you have to have a consensus and——

Dr. **SANDWEISS**. Yes, essentially.

Mr. **CARNEY**. That's great. I can understand that.

Dr. **SANDWEISS**. Yes.

Mr. **CARNEY**. But then I turn that around and say that we have to provide to the American public a consensus that what the high-energy physics community is doing is correct and is right and that their taxpayer dollars are being spent in a prudent fashion.

Tell me, Dr. Sandweiss, how do I go back to the people in the First Congressional District in New York and tell them that it is prudent to abandon a $200 million concrete doughnut in that district to move on to build a machine that I can't even describe.

I don't know if it will be 50 kilometers. I don't know if it will cost $8 billion. I don't know where it is. I don't know who heads up the team. I don't know the design concept.

How do I get a consensus of the American taxpayer that this is a prudent step? Could you answer that question for me?

Dr. **SANDWEISS**. It is difficult. But if it's—the reason—the reason is really as I said. I mean, it's very unfortunate for the CBA project itself that the—the advances of physics—I mean, in a sense, at the time of the Trilling report, for example, the European P-P Collider had not worked and it was still a fairly complicated scheme. It was being constructed but it hadn't worked. In fact, it worked magnificently and the W and the Z were discovered. Those were two of the main targets of the CBA program.

Mr. CARNEY. Does——

Dr. SANDWEISS. So, maybe just to finish this, I think all that one can honestly say is that it's—unfortunately, the machine as it had been planned would not have been a prudent use of the taxpayers' dollars because of the changes which have taken place in those—it's a sad thing to happen sometimes; that's what happened.

Now, on the other front, I would hope that we would not lose—I mean, there are many uses for the facility, certainly for the equipment as Stan commented, and as I commented briefly, I would hope very much that the—in the end when one looks back at this perhaps 10 years from now, one will see that this investment, indeed, has borne fruit. That is my personal hope in any case.

Mr. CARNEY. Wouldn't it be more prudent to find something for the investment that the American taxpayers have made at Brookhaven now before moving on to this dream machine?

Dr. SANDWEISS. Well, that may not be a matter totally for high energy physics.

Mr. CARNEY. I can appreciate that——

Dr. SANDWEISS. Right. And so I——

Mr. CARNEY. But collectively the wisdom that the Department of Energy, I am sure, could come up with an answer to that solution.

Dr. SANDWEISS. Well——

Mr. CARNEY. You know, you mention that the discoveries that CERN would dictate that we should move forward.

Dr. SANDWEISS. And other things.

Mr. CARNEY. Now, was it not true that CBA would be working on the W?

Dr. SANDWEISS. Certainly. In fact——

Mr. CARNEY. Is it—is it——

Dr. SANDWEISS. It would—it would have done detailed work on the properties of the W and the Z.

Mr. CARNEY. Is it not true that SLAC is doing work on the Z?

Dr. SANDWEISS. Well, will be, hopefully when it is——

Mr. CARNEY. Well, why does it hold true that we stop this project on the W and continue the project that we haven't even broken ground for on the Z when CERN made the discoveries? How do I explain that to the person in the street?

Dr. SANDWEISS. Well, the Z, which was discovered at—at CERN was on a hadron collider. A hadron collider produces particles in a way which is very different from an electron/positron collider.

The ability to study certain kinds of particles, when you can make them in an e plus-e minus collider, is very much more detailed and very different. In fact, if you look at the U.S. program you will see that we have overlapping electron machines and proton machines continually. For example, we have charm production at Fermilab and charm production at the Spear Accelerator and the PEP Accelerator at Stanford.

The two methods, e plus-e minus and hadron collisions, are complimentary methods of studying particles. And so the trouble with the CBA was that it was the same kind of machine as the European machine.

The SLC is a different kind of machine. It is an electron kind.

Mr. **Carney**. Then I suggest that the—the report, the Woods Hole report, changed its language a little bit because it says in its conclusions on page 26, that the physics goal to study—is to study of the Z physics just as at LEP.

To me it indicated—to me that——

Dr. **Sandweiss**. You are speaking of the CBA?

Mr. **Carney**. Yes.

Dr. **Sandweiss**. Yes.

Mr. **Carney**. No, no. I'm—I'm speaking of your report on SLC.

Dr. **Sandweiss**. Yes.

Mr. **Carney**. And it says in there that the physics goal is study of the Z physics just as at LEP. So——

Dr. **Sandweiss**. Oh, for the SLC it is similar to LEP?

Mr. **Carney**. Right, that's right.

Dr. **Sandweiss**. Well, the SLC will be finished 2 years before LEP.

Mr. **Carney**. But why continue? You haven't even broken ground. You have given up on the CBA because you are studying the W when we've got it at CERN. Now, according to——

Dr. **Sandweiss**. Do you want to discuss the SLC? We could, I mean, somewhat, if you like. I mean, the SLC has——

Mr. **Carney**. Well, I did—what I—what I'm discussing, Dr. Sandweiss, is the consistency. That's what I talked of in my two opening statements.

Dr. **Sandweiss**. Yes.

Mr. **Carney**. We are not being consistent saying let's break ground at SLC and go forward and study the physics of Z just as as LEP, quote the report. And then say we shouldn't go along with CBA because they are going to study the W, which was found at CERN.

Dr. **Sandweiss**. Yes, well, there are several differences. I would be happy to discuss them some, if you like, and others——

Mr. **Carney**. All right.

Dr. **Sandweiss** [continuing]. Can add to it. One important distinction is that the SLC is a new kind of accelerator, which has never been built before. It is a linear collider rather than a circular collider. The linear collider involves all kinds of new technology in the accelerators. It involves beams whose dimensions are millionths of a meter. Micron diameter beams, it involves control problems, but it—which are all very new. So a great part of the SLC—if you read the Trilling report you will see this explained in great detail—is, in fact, that it will advance the accelerator art.

It is very different from LEP. LEP is a round machine in that sense.

Second, the SLC does have the potential of being there several years before LEP, whereas, the CBA has already been scooped, if you like, by the European machine which will have been running 4 or 5 years before the CBA would have come on.

Mr. **Carney**. We are in a race mode again. You know, one machine——

Dr. **Sandweiss**. No, but you were asking why is one different from the other. I mean——

Mr. **Carney**. OK.

Dr. **Sandweiss** [continuing]. I was explaining the——

Mr. **Carney**. Well, I——

Dr. **Sandweiss** [continuing]. Difference of the SLC——

Mr. **Carney** [continuing]. Then I simply suggested that we change the language in the report——

Dr. **Sandweiss**. Perhaps it should have been more——

Mr. **Carney** [continuing]. Because it indicated to me right out of the report that it's just as in LEP. That's what the report said.

Dr. **Sandweiss**. That is misleading because it is, of course, a different technique——

Mr. **Carney**. And you asked if I had read the Trilling report. I would like to inform you, I think I've read every one of these cover to cover and the chairman informs me that at the end of this Congress I'm getting a Ph. D. in physics. [Laughter.]

Dr. **Sandweiss**. Wonderful.

Mr. **Carney**. If I may, Mr. Chairman, with your indulgence, I have a couple of other questions that I would like to ask.

On the Trilling report, Dr. Drell, then the Chairman of the High Energy Advisory Panel, sent a Department of Energy report, the HEPAP Subpanel on Long Range Planning for the U.S. high energy physics program—chaired by Dr. Trilling. And the cover letter, in which he stated that the Subpanel concluded that it is essential for an active and healthy U.S. program in the early 1990's, that a major new facility for experimentation be available by the end of this decade.

If the present ISABELLE project, which is now the CBA—if the present Isabelle project must be discontinued, then another major facility must be started in the mid-1980's so it is available for research by 1990.

HEPAP strongly agrees that such a new facility is essential by the end of the decade. What—how do we fill in that new facility?

Dr. **Sandweiss**. Well, you're—you're asking me?

Mr. **Carney**. Yes, Doctor, I'm sorry.

Dr. **Sandweiss**. One of the major worries about the recommendation that we made—one of the risk factors in it—is the fact that if we proceed the way that we are going, if we do proceed on the SSC—the earliest it could be is sometime early in the nineties. And so we wouldn't have that new facility. And we would—we would be running the risk, then—would be running a risk to the--to the vitality of the field to the opportunities that people have to work to the whole—the whole character. We realize that.

There are reasons why we might be in a somewhat better position than we previously thought but it is still a serious worry. One of the reasons is that the experiments on the SSC will clearly be very elaborate and—and complicated, and will require a lot of preliminary design, testing, and construction.

And so we anticipate that even though the SSC might not come on until the midnineties, early, hopefully, you know, between 1990 and 1995, the work on the experiments would have to begin quite a bit earlier. And that would be one of the things that would be important in the vitality of the field. That's one reason.

I think the Trilling panel, when they thought of that new facility, they weren't thinking of something coming a few years later. And so preparing for that would be one thing.

Another thing, certainly, is—is the fact that—that the experiments in general have been getting more complicated and involving larger groups of people, and—it seems to be somewhat less of a worry than it was—it has changed rapidly. I mean, it's been changing rapidly—the sociology. And so, I think the situation was a bit more worrisome at the time—the deliberations of the Trilling panel, which, of course, were more than 8 to 10 months ago.

Then, finally, I would say that we do have, of course, some collaboration with the Europeans—a substantial number of Americans do use the European facilities, and we do expect that that will contribute as well to the opportunities which we have for our scientists.

Mr. CARNEY. Thank you.

The gentleman asked that I yield for a minute. I'd be glad to.

Mr. BATEMAN. I thank the gentleman very much. Unfortunately, despite my interest in this matter, I've just got to get to another subcommittee meeting with a markup that started a long time ago.

I would simply ask, Mr. Chairman, for leave, if I might submit some written questions which I would like to have responses to but don't have time to remain and to elicit answers to today.

I thank the witnesses for their testimony. It has been a very stimulating morning, but there are questions I would like to submit in writing and would appreciate having response to. If that's agreeable, I thank the Chair.

Mr. BOUCHER [presiding]. Without objection, that is permissible. Mr. Carney.

Mr. CARNEY. Thank you.

Dr. Sandweiss, you said that SSC would be on line between 1990 and 1995; is that what you said?

Dr. SANDWEISS. Well, hoping, that was the—that was the plan; the hope that it would be able to be, yes.

Mr. CARNEY. What happens if it's not?

Dr. SANDWEISS. Well, I guess it would depend—well, first of all, it wouldn't, of course, just suddenly not be there. The first thing that would happen——

Mr. CARNEY. Well, you know, if it's——

Dr. SANDWEISS. I was explaining how I thought it might go. The first thing that would happen is that we would be getting the results from the R&D program. That program, hopefully, will begin very soon with your agreement. And if those results are, for example, negative—I don't believe that's likely to be the case, but it could certainly be—then we would begin to have some warning that this was not going to be a good idea, if that were really the case.

Mr. CARNEY. How long would that take?

Dr. SANDWEISS. Well, the R&D program we estimated could be completed in 3 to 4 years. Early results would undoubtedly come along the way—it might be very positive results in a rather short time. That would be, I hope, the most likely outcome.

Mr. CARNEY. Three to four years of——

Dr. **Sandweiss**. Three to four years would be at that point, we believe, it would be possible to have a well-defined machine designed with a reliable cost estimate which could go forward as a real proposal for construction. This is now in the stage of R&D; not a proposal for construction.

Mr. **Carney**. I appreciate that. By the way, in that timeframe you could do both and CBA would be ready to go—the proposal is for 197—87.

Dr. **Sandweiss**. Yes, yes. That—those two things conflict head on. That's correct.

Mr. **Carney**. Right. And then given that the R&D goes well, 3 to 4 years, we are talking about 1987, construction of how many years?

Dr. **Sandweiss**. People speak of five or six, that kind of time.

Mr. **Carney**. Six years—1993. Now, I appreciate what you are saying and as I said in my opening statement, I think that they are rather optimistic timeframes. And if I may, I would just like to, for the record, explain my feelings on that.

If you were to take the colliding beam accelerator, which is 2.4 miles in circumference, and you try to extrapolate the machine that I've heard would be anywhere between 30 and—to 50 miles in diameter, and take the timeframes and the costs that have been associated with that machine, there are some similarities—we are going to the next, the leading edge of our capabilities, our intellectual capabilities, our scientific capabilities. And it's exciting, it's stimulating. And I agree, that's where we should always be.

But we don't always find that answer so we can move along and move along and move along. And it becomes a matter of more time, and it's understandable; and more money, and it's understandable.

If we build a machine of one size and it costs the taxpayer $500 million to complete, and that's in the fiscal year moneys that you were building it in, from 1977 to 1987, let's say, $500 million—you have a 400 by 400 GeV, 2½ miles long.

And now we're going to a—let's say for argument's sake, a 60-mile tunnel that will be 10 or 20 TeV. And I just try to put $500 million, and I am very confused how you come up with $2 billion on the other.

Dr. **Sandweiss**. I would be happy to speak to that a little if you like.

Mr. **Carney**. I would love that; yes, I would appreciate that.

Dr. **Sandweiss**. OK. I—first of all, the advances that have been made in the course of the Fermilab project and the CBA project—ISABELLE—are—we have spent an enormous amount of energy, time, and effort in learning about the technology of superconducting magnets.

And I believe that what we have acquired is a technology and engineering base that we can now exploit. This is not just my belief, but it is widely the belief of everybody who works in this area.

So, hopefully—the first answer is we don't have to make all the old mistakes again.

The second answer is we would not attempt to raise the field level appreciably. That is, it would be from 10 to 20 percent within the field levels that have already been achieved.

What we would aim at is not a new invention from the point of view of capability of the magnet but, rather, take the base we have and now start doing the engineering which makes it cheaper. And there are many good ideas. In fact, one of the best ideas came, in fact, from Brookhaven as part of the CBA project but it was eventually decided not to build the CBA that way, and that was the 2-in-1 concept at Brookhaven, in which you put two magnets in one cryostat and possibly even put them essentially in the same iron. That saves you almost a factor of two right off the bat—not quite. And that's—that has been, in fact, the first such magnet that was built at Brookhaven.

Mr. CARNEY. I appreciate that. I've seen it, and I can appreciate what you're saying.

Dr. SANDWEISS. Yes.

Mr. CARNEY. But let's get to something that's more static.

Dr. SANDWEISS. One other thing I just wanted to say and that was that people don't generally realize that when the energy of the beam increases, it undergoes a funny kind of shrinking kind of operation. All beams do—they just get smaller. And the fact that they get smaller means that the aperture of the magnet just naturally can be smaller, with no inventions. It just—if you look at all accelerators, the low-energy ones are big, and the high-energy ones are littler. And this one will be like that. So there's just a lot less pounds of magnet in this thing. And that's all part of it.

We really have, I think, good scientific reasons for expecting to reduce the cost, but we must demonstrate it with the program.

Mr. CARNEY. OK. Let's talk about something that I think everyone in this room will agree is somewhat static. If it's not static it increases in cost, and that's bricks and mortar—nothing fancy, nothing inside the tunnel.

If you build a tunnel for $120 million, that is 2.4 miles long, you are already talking close to $1 billion for bricks and mortar——

Dr. SANDWEISS. If you scale it.

Mr. CARNEY [continuing]. On the dream machine that I don't really know how big it is going to be. But there seems to be some kind of consensus that it would be——

Dr. SANDWEISS. It's big.

Mr. CARNEY [continuing]. It's 50 miles, 30 miles in diameter—so my math isn't quick enough to figure the circumference going from a 30-kilometer or 30-mile diameter. But it is big and we would have to, in my quick mathematical calculations, would have to be in excess of $1 billion for the bricks and mortar.

Dr. SANDWEISS. Yes.

Mr. CARNEY. The next thing is we built that particular machine on Government-owned property. And since we are the keeper of a third of this country's lands, we might be able to find Government-owned property to put it on. But we might not be able to find that type of property and, therefore, we are talking about at least the rights to go on someone's property, put a tunnel underground, and, you know, then let them go back.

I would only suggest, Dr. Sandweiss, that we try to do that with coal slurry and it didn't pass Congress because of eminent domain. Of course, the Federal Government doesn't need eminent domain, they have it.

But the problems associated with that—let's say Joe the ragpicker in the middle of the desert decides that he's not going to let Uncle Sam put his tunnel through his quarter acre, and you can't go around it—he holds you up for a year in litigation or something.

So we have a time problem——

Dr. SANDWEISS. Yes.

Mr. CARNEY [continuing]. And we have a problem of cost. So I must admit I think that those figures are most optimistic, both in a time frame and in the cost of building the new machine. But, frankly, no one knows what it is.

Maybe Dr. Wojcicki——

Dr. SANDWEISS. May I just comment? One more comment about what you said, quickly. One thing I would like to point out—the reason why the diameter is unknown is because we don't know which diameter is cheapest. It isn't that it will be a certain diameter or another diameter, and then we have to estimate the cost—you could build a machine right now at several different diameters. But what we are trying to discover is which one is the cheapest. And that's a trade off between the problems you mentioned and the problems of what you put in the tunnel. And it is that engineering system study which is partly being done, but will be done by the R&D.

But the other thing is the tunnel will be much, much smaller than the CBA tunnel, partly for the reasons I earlier mentioned, and partly because of engineering advances whereby you will not have to put in the tunnel the services which currently go in the CBA tunnel. Those—the idea will be to try to fabricate these magnet systems so that to some extent they contain their own services, so one can—you do engineering.

It is just like automobiles, once you've invented the automobile, you might say, well, that's all there is, but it's certainly true that a modern automobile is very different from a 1920's automobile.

Mr. CARNEY. That's a very good point that you make—the tunnel will be smaller——

Dr. SANDWEISS. Much smaller, yes.

Mr. CARNEY [continuing]. Because driving in here—and I apologize to the Chair, and I don't think I apologized to the public, and I should, for being here late. It seems that every time it rains, the time it takes to come down 395 increases geometrically. And I left my home in ample time to be here but, unfortunately, it took 1 hour and 25 minutes to come from Duke Street to here.

The point that I am trying to make is that if we have a tunnel at 60 miles in circumference and there's a problem 30 miles out, the mechanic, or the engineer, or the physicist, has to get out to that. So we have a transportation problem right now.

Dr. SANDWEISS. Yes.

Mr. CARNEY. We have to build in some type of transportation.

My thought was that this tunnel might have to accommodate a subway or something of that nature so you could get to your work station.

You know, one fellow is 30 miles over here doing physics, the other fellow is 30 miles over there doing physics; the lunchroom, hopefully, will be conveniently in between, or the hotels that they are staying at would be convenient. If not, we have to have this huge transportation network. We can't have a Holiday Inn at every instrumentation station. All right?

So, you know, although I thought we would have to probably do this internally—have a transportation network; nothing real fancy. But I don't think it would be smart to have scooters in there, you know.

But I was thinking it would have to be bigger. You bring a point up that I didn't recognize—it could be smaller. And, indeed, I'm pleased to see that. But I would suggest it might be bigger.

I would like to, Mr. Chairman, just summarize.

I would like to first thank the three of you. I think you all recognize that I have considerable differences of opinion than you do, and I appreciate all three of you listening to my questions and answering them.

And I think in summary I would simply like to say that, again, we have to come back to this consistency. Your community—if it's to be successful and, indeed, I hope it will be successful—has to demonstrate not only to the Congress but to the American taxpayer that spending billions of dollars to understand what this little particle does is important. We recognize that.

It's difficult to convey that message to people who are perhaps unemployed, or who are losing their jobs, that this is important; that we have to go out and find out the reasons.

There are a unique group of people that are stimulated to go out and ask the questions why and seek the answers to the whys.

But the great majority of the American people need the confidence that indeed this is right. And frankly speaking, the demonstration of the high energy physics community, in their inability to be consistent, in their inability to make a decision; and the fact that they had to look at a project five or six times, it becomes confusing to that American public.

And then when they see that community decide with one panel that was almost equally divided, that we should stop a program that their hard-earned tax dollars have been invested in, and move on to something that I can't describe, the distinguished panelists can't describe, the chairman of the committee can't describe—no one knows what we're really moving on to.

It is a difficult thing. It makes it rather difficult for us as guardians of the Treasury to make decisions.

So I would hope that we can find some consistency in what we do.

And lastly, I would hope, and I would strongly urge that we readjust our times; we readjust the schedule that we've come up with; and we look first at what's the prudent thing to do with a fantastic facility that the taxpayers have already paid for.

I don't think the people would be too upset if we said we're going to use this facility for research that will improve our lot. And again we are going to put on a fast track an R&D program to build a better facility, a bigger facility, that will answer those questions that are a little further out in the darkness of our knowledge.

I don't think they would disagree with that if we took care of the first problem—if we said, hey, we have a huge facility already in place; it has the necessary equipment to do research, whether it be a proton-proton colliding beam accelerator, or whether it be a heavy ion facility, whatever; let's not go forward and give the perception that the community is flippant, noncaring, to the American public's tax money. It will be a mistake. It will not help to unify the Congress, who is going to be asked to support these projects.

I would suggest to all three of you very strongly that it would make a very easy case for me to go to many of these organizations; to travel around the Hill, talking about pork barrel projects and that type of thing—talking about the Clinch Rivers, and the Tennessee-Tombigbees. It would make a very easy case for me to get 218 rather nervous Congressmen to support an unknown project and abandon a $200 million commitment.

So you have a difficult task if we don't take care of that commitment. I want to act responsibly, and I know that the high energy physics community does. I must admit, and I would say publicly that Dr. Trivelpiece has been more than patient with this one Congressman, and I know that his feelings are that we have to act responsibly with the taxpayers' money.

I suggest the way to do that is to change the timeframe; take care of the one problem and move on to the next problem. Then we will have guarded the taxpayers' investment and we will have brought some consistency to the high energy physics community.

Mr. Chairman, in conclusion I would like to thank you for being so generous with your time and ignoring the 5-minute rule today. [Laughter.]

Mr. CARNEY. Mr. Chairman, if I may, I would like to ask unanimous consent that the statement of Mr. Sensenbrenner be submitted in the record immediately after your opening statement.

Mr. FUQUA. Without objection.

Mr. CARNEY. Thank you, Mr. Chairman.

Mr. FUQUA. I want to thank Dr. Trivelpiece, Dr. Wojcicki, and Professor Sandweiss for being here this morning. I think you have contributed greatly to a very, very difficult situation that we are all very concerned about. And we hope that our ultimate decision is the right decision. Thank you very much for being here, gentlemen.

Dr. TRIVELPIECE. Thank you.

Dr. WOJCICKI. Thank you.

Dr. SANDWEISS. Thank you.

Mr. FUQUA. The meeting is adjourned.

[Whereupon, at 11:40 a.m., the subcommittee was adjourned.]

APPENDIX I

QUESTIONS AND ANSWERS SUBMITTED FOR THE RECORD

DON FUQUA (Fla.), Chairman

ROBERT A. ROE, N.J.
GEORGE E. BROWN, JR., CALIF.
JAMES H. SCHEUER, N.Y.
RICHARD L. OTTINGER, N.Y.
TOM HARKIN, IOWA
MARILYN LLOYD, TENN.
DOUG WALGREN, PA.
DAN GLICKMAN, KANS.
ALBERT GORE, JR., TENN.
ROBERT A. YOUNG, MO.
HAROLD L. VOLKMER, MO.
BILL NELSON, FLA.
STAN LUNDINE, N.Y.
RALPH M. HALL, TEX.
DAVE MC CURDY, OKLA.
MERVYN M. DYMALLY, CALIF.
PAUL SIMON, ILL.
NORMAN Y. MINETA, CALIF.
RICHARD J. DURBIN, ILL.
MICHAEL A. ANDREWS, TEX.
BUDDY MAC KAY, FLA.
TIM VALENTINE, N.C.
HARRY M. REID, NEV.
ROBERT G. TORRICELLI, N.J.
FREDERICK C. BOUCHER, VA.

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WILLIAM CARNEY, N.Y.
F. JAMES SENSENBRENNER, JR., WIS.
JUDD GREGG, N.H.
RAYMOND J. MC GRATH, N.Y.
JOE SKEEN, N. MEX.
CLAUDINE SCHNEIDER, R.I.
BILL LOWERY, CALIF.
ROD CHANDLER, WASH.
HERBERT H. BATEMAN, VA.
SHERWOOD L. BOEHLERT, N.Y.
ALFRED A. MC CANDLESS, CALIF.
TOM LEWIS, FLA.

J. H. POORE
Executive Director
ROBERT C. KETCHAM
General Counsel
DAVID S. JEFFERY
Minority Staff Director

October 27, 1983

Alvin W. Trivelpiece, Director
Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece: Al

Thank you for your excellent testimony before the Subcommittee on Energy Development and Applications on October 19, on the Department of Energy's high energy physics facilities. Attached are some questions that Members of the Subcommittee would like you to answer, in order to supplement the record of the hearing on this topic.

I would appreciate your responses to these questions by November 15, 1983. If you have any questions regarding this request, please contact Dr. Harlan Watson of the staff of the Subcommittee on Energy Development and Applications at (202) 225-4320.

Sincerely,

DON FUQUA
Chairman

DF:Wcw
attachment

Additional Questions for Dr. Alvin W. Trivelpiece

1. If the $5 million in CBA construction funding in FY 1984 is reprogrammed to a project providing Brookhaven with a heavy ion capability, what additional level of R&D funding would be appropriate?

2. What heavy ion capability will Brookhaven possess if the Tandem Van de Graaff is linked to the AGS? How does this compare with the capability of existing heavy ion facilities, such as the Bevalac at Lawrence Berkeley Laboratory?

3. What aspects of high energy or nuclear physics can be addressed with a relativistic heavy ion collider?

4. Is there a firm, scientific basis and an overwhelming scientific consensus as to the energy range and size nuclei desired for a significant relativistic heavy ion facility?

5. What commitment of money and facilities might be required to construct a forefront relativistic heavy ion collider?

6. Are there alternative concepts, perhaps requiring technological breakthroughs, which would allow access to the multi-TeV energy regime without such a gigantic machine as SSC is currently configured?

7. What are your current estimates of how long it will take; and how much money it will cost to arrive at the point at which you will have a fixed design concept and reasonable estimates of construction costs for the proposed SSC?

8. What are the main problems you need to address in the planning and R&D period before a decision could be made to construct the SSC? Are there any technological or cost breakthroughs necessary in order to make this project viable?

9. Assuming an R&D program and planning for this facility were approved, how do you plan to go about organizing this effort?

    - Are sufficient personnel available? Where?

    - What kind of organizational structure do you envision?

    - What approximate size would you expect an organization to be?

    - What is your general timetable for proceeding and what are your major milestones?

10. On page 8 of his testimony, Dr. Wojcicki argued that the SSC should be a hadron-hadron collider, of which Tevatron I will be the prototype, rather than a lepton-lepton linear collider, of which the SLAC Linear Collider (SLC) at Stanford will presumably be the prototype.

    - Is this a correct interpretation?

    - Aren't Tevatron I and the SLC due to finish the construction phase and begin operation in the 1986-1987 time frame?

    - If Tevatron I and SLC operate to currently predicted specifications, won't this open the way to much higher energies for both hadron-hadron colliders and for lepton-lepton linear colliders?

    - Won't the increase in energy of Tevatron I to the 10-20 TeV regime be roughly equivalent to the increase in energy of the SLC to the 1-2 TeV regime?

    - Might it not be premature to concentrate totally on a 10-20 TeV hadron-hadron collider at this point? Wouldn't it also be wise at this stage to consider a 1-2 TeV lepton-lepton linear collider?

11. What long term effect will the SSC, if constructed, have on the usefulness and longevity of other existing U.S. high energy physics accelerator facilities?

12. With regard to the physicists involved in ongoing high energy physics activities, what adjustments will be necessary to experimental programs and facility utilization because of the long period of time in which no new U.S. facilities will be constructed?

13. Siting of a facility such as SSC has already attracted a lot of attention in the media. What are your contemplated procedures for addressing the siting issue?

14. We have heard SSC cost construction estimates ranging from under $2 billion on the optimistic side to $8-$9 billion on the pessimistic side.

    - What maximum cost level do you think can be tolerated for the construction of the SSC?

    - If Phase I cost estimates of the SSC exceed this maximum level, what alternatives exist?

15. The July 1977 issue of Reviews of Modern Physics contained an article by H. Hahn, M. Month, and R.R. Rau titled "Proton-proton intersecting storage accelerator facility ISABELLE at the Brookhaven National Laboratory." On page 627 of this article, it is stated: "Detailed work on the ISABELLE design began at Brookhaven in May 1971." Please provide, by fiscal year, from FY 1971 through FY 1983, the following information for the ISABELLE/CBA project:

    - Operating expenses
    - Capital equipment expenses
    - Construction funding.

    Please delineate those funds expended at Brookhaven National Laboratory and those funds expended at other institutions.

16. You explained why the Colliding Beam Accelerator (CBA) at Brookhaven National Laboratory is apparently no longer considered a viable facility in the High Energy Physics program.

    - What effect does suspension of construction of the CBA at Brookhaven have on the overall high energy physics program in the U.S.?
    - What effect does it have on Brookhaven programs and personnel?

17. The Subpanel report notes that the SLAC Linear Collider (SLC) at Stanford: a) will have physics objectives similar to those of the LEP machine at CERN in Europe; b) will have a luminosity inferior to that of the LEP; c) will have one experimental region versus six experimental regions for LEP; and d) will operate in an energy regime already being explored by the CERN SPS collider and which will be explored later by Tevatron I and LEP.

    - If these points are reasonably accurate, could it not be argued that the SLC will not be performing at the forefront of high energy physics?
    - On physics grounds alone, couldn't one argue that it would be just as logical for the Subpanel to have recommended cancellation of the SLC as the cancellation of the CBA?

18. At the Summer 1982 Snowmass meeting of the American Physical Society Division of Particles and Fields, Nobel Laureate Burton Richter of SLAC gave a talk titled, "The Future of Electron-Positron Colliders." During this presentation, he listed beam parameters near those of the SLC for a 1 TeV by 1 TeV electron-positron collider, and noted: "It is certainly not

feasible to simple [sic] extend standard linacs like SLAC, for if we did, we could have a total length of machine of about 60 km a power consumption of gigawatts, and a cost of $\$2 \times 10^9$. However, if we can make energy efficient, low cost per unit length accelerators, the world high energy physics community can afford such a machine at its present budget level. We don't know the best way to build such a machine and an intense R&D program will be required to determine the best road."

- Is the SLC project developing energy efficient, low cost per unit length accelerators, or is it relying primarily on existing SLAC linac technology?
- If the SLC is relying primarily on existing SLAC linac technology, then isn't it hard to argue that SLC is really developing the technology of electron-positron linear colliders "which may open the way to much higher energies"?
- On accelerator technology development grounds alone, isn't it hard to argue that the SLC should be built?

19. Section V.F. of the January 1982 "Report of the Subpanel on Long Range Planning for the U.S. High Energy Physics Program of the High Energy Physics Advisory Panel" (i.e., the Trilling Panel report) includes comments on the individual subsystems and technical areas associated with the SLC project. These include the following:

    - Cost and Schedule
    - Project Management
    - Damping Ring, 1 GeV Buncher Section, and Arc Structure
    - SLC Positron Source
    - Klystron R&D and Various Energy Options
    - Beam-Beam Interaction
    - Final Focus
    - One-Sector Results
    - Buncher-Injector Systems
    - SLC R&D Program
    - Performance Improvement and Contingency
    - Accelerator Physics

Please update the Trilling Panel report comments for each of these areas. Please indicate changes from those comments given in that report, including reasons for these changes.

20. Please answer the following questions concerning the Fermilab Energy Saver Project (Project 79-9-b):

    - When was construction of the project completed?
    - When did operation of the device begin?
    - How does the current performance of the Energy Saver compare with the performance predicted prior to construction?
    - What difficulties have been encountered in the initial operation of the Energy Saver, and how are they being overcome?
    - Total construction cost for the project were estimated to be $46.6 million, including $28.8 million for magnets, $4.4 million for refrigeration, $1.4 million for power supplies and controls, $0.4 million for rf, $0.1 million for extraction, $2.2 million for special facilities, $0.1 million for conventional facilities. and $6.8 million for contingency. Please provide the final costs for each of these construction cost categories, and explain any variance between the final costs and the estimates.
    - Other project funding was estimated to total $52.043 million, including $50.043 million for R&D and $2.0 million for other project related costs. Please provide the final costs for each of these categories, and explain any variance between the final costs and the estimates.

21. Please answer the following questions concerning the Tevatron I project (Project No. 81-E-218):

    - What is the construction status of the accumulator and debuncher rings?
    - What is the construction status of the two experimental-detector areas?
    - Is Tevatron still within the estimated construction budget of $82.5 million? If so, is the $13.75 million in contingency still available? If not, what explanation is there for additional cost overruns? Is the project on schedule?

- Have any technical difficulties been encountered with magnet fabrication and performance, refrigeration upgrade, or rf cavity fabrication? If so, please describe the difficulties and corrective measures being taken.

- Are the direct R&D operating costs within the $38.968 million estimated? If not, please explain the reasons for any variances.

- What major items of equipment are included in the $2.889 million capital equipment budget? Will all the required capital equipment be provided for $2.889 million, or will additional funding be needed and if so, how much and for what reason?

22. Please answer the following questions concerning the Tevatron II project (Project No. 82-E-206):

    - What is the construction status of the extraction system?

    - What progress has been made in upgrading the external beam switchyard to 1 TeV?

    - What is the status of the 1 TeV target assembly and experimental stations?

    - What is the status of the new secondary beam lines (and beam line replacements) in the external experimental areas? What percentage of the equipment has been completed and what progress has been made on the support facilities?

    - Will Tevaton II construction be completed within the original schedule for $49.0 million or less? If not, please explain the reasons for any expected schedule and/or cost overruns.

    - If the project is still within budget, how much of the $7.8 million contingency remains uncommitted? How much of the other project-related costs and direct R&D of the estimated $18.0 million remains uncommitted?

23. Please provide 1) a listing of all reports by the High Energy Physics Advisory Panel since its establishment in January 1967; and 2) two copies of each of these reports.

Department of Energy
Washington, D.C. 20585

December 2, 1983

Honorable Don Fuqua
Chairman, Committee on Science
  and Technology
House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

On October 19, 1983, Director Alvin W. Trivelpiece appeared before the Science and Technology Subcommittee on Energy Development and Applications to discuss the Department's high energy physics facilities.

Following that hearing, you submitted written questions for responses to supplement the record. Enclosed are the answers to those questions.

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

Thomas H. Pretorius
for Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

✓cc: Ms. Christine Wegman
Subcommittee Staff
(without attachments)

POST-HEARING QUESTIONS AND ANSWERS
FROM THE HEARING OF
ALVIN W. TRIVELPIECE
DIRECTOR
OFFICE OF ENERGY RESEARCH
ON
OCTOBER 19, 1983
BEFORE
HOUSE SCIENCE AND TECHNOLOGY
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS

Question 1: If the $5 million in CBA construction funding in FY 1984 is reprogrammed to a project providing Brookhaven with a heavy ion capability, what additional level of R&D funding would be appropriate?

Answer: For the specific heavy ion project presently proposed [linking of the Tandem Van de Graaff Facility with the Alternating Gradient Synchrotron (AGS)], Brookhaven requests $9,130,000 in construction funds and $2,975,000 in construction-related R&D operating funds. Thus, completion of this heavy ion project would require $4,130,000 in construction funds in addition to reprogramming of $5,000,000 of Colliding Beam Accelerator (CBA) construction funds and $2,975,000 in construction-related R&D operating funds.

Execution of the initial round of experiments with AGS heavy ion beams requires modification of existing experimental area equipment and fabrication of new instrumentation at an estimated cost of $5,500,000 in FY 1985 and FY 1986 capital equipment funds. Upon completion of the project, Brookhaven estimates that an incremental $1,900,000 per year operating funds will be required for conduct of heavy ion experiments and for operation of the transfer line linking the Tandem Van de Graaff Facility to the AGS. Incremental operation of the AGS (that is, in addition to the present level of AGS accelerator facility operation for non-heavy ion research) for heavy ion nuclear research is estimated at $500,000 per week.

Question 2: What heavy ion capability will Brookhaven possess if the Tandem Van de Graaff is linked to the AGS? How does this compare with the capability of existing heavy ion facilities, such as the Bevalac at Lawrence Berkeley Laboratory?

Answer: The proposed Brookhaven project is designed to provide beams of heavy nuclei varying in mass up to 32 atomic mass units (e.g., sulfur) with energies up to 15 GeV per atomic mass unit. Less intense beams up to 40 atomic mass units (e.g., calcium) are believed possible.

The Bevalac at Lawrence Berkeley Laboratory presently provides beams of heavy nuclei varying in mass up to 40 atomic mass units with energies up to 2 GeV per atomic mass unit. Beams of useful intensity up to 197 atomic mass units (e.g., gold) are now available at Bevalac with energies up to 1 GeV per atomic mass unit. Also, at relatively low intensity, Bevalac has a uranium (238 atomic mass units) beam at 0.96 GeV per atomic mass unit. The existing Bevalac beam capabilities are substantially higher in mass, but substantially lower in energy, than the proposed Brookhaven capabilities.

Thus, linking of the Tandem Van de Graaff Facility to the Alternating Gradient Synchrotron would provide an important research capability which complements, rather than duplicates, Bevalac research capability.

Question 3: What aspects of high energy or nuclear physics can be addressed with a relativistic heavy ion collider?

Answer: In the form of quantum chromodynamics, we believe that we may be approaching a fundamental theory of the strong nuclear force. Many aspects of present high energy and nuclear physics research focus on investigations of the properties and interactions among quarks and gluons and their descriptions in terms of quantum chromodynamics. However, in all experiments so far, free or unconfined quarks have not been observed. All our present knowledge is based upon experiments in which quarks have been confined inside neutrons, protons, mesons, and other hadrons. Much of the present enthusiasm for heavy ion collider research centers upon the expectation that interactions among unconfined quarks and gluons may be deduced from heavy ion collider experiments.

In addition to investigations of quark-gluon plasmas, there is scientific enthusiasm for systematic study of the behavior of nuclear matter at high excitation and compression. For example, Bevalac experiments show that selected products of heavy nucleus collisions are many times more reactive than normal nuclei. Almost certainly, this phenomenon will require experiments at higher energies for a complete understanding. Also, for example, nuclear theorists speculate that transition of nuclei to a new, ordered state (analogous to ferromagnetism in atomic physics) is possible in heavy nucleus collisions.

Question 4: Is there a firm, scientific basis and an overwhelming scientific consensus as to the energy range and size nuclei desired for a significant relativistic heavy ion facility?

Answer: There is a growing consensus among high energy and nuclear physicists that research with colliding heavy nucleus beams is an important thrust in high energy and nuclear physics. However, at this time, there remains considerable discussion among leading scientists as to optimal mass and energy parameters.

At its July 11-15 Long Range Plan Workshop, members of the DOE/NSF Nuclear Science Advisory Committee (NSAC) and other members of the scientific community discussed the scientific basis for research with colliding beams of massive nuclei. In a letter of August 4, 1983, to J. E. Leiss (Associate Director for High Energy and Nuclear Physics, DOE) and M. Bardon (Director, Physics Division, NSF), Dr. John Schiffer (Chairman, NSAC) reported:

> "From the consideration of several new and exciting scientific opportunities for the future, the Workshop identified one that will open a new frontier of many-body, strong interaction physics: an accelerator, capable of producing colliding beams of very heavy ions at energies of about 30 GeV per nucleon...
>
> "It is the opinion of this Committee that such a facility should be built by the United States expeditiously..."

Question 5: What commitment of money and facilities might be required to construct a forefront relativistic heavy ion collider?

Answer: We are at an early stage in the process of deciding upon key beam parameters such as energy range, mass range, luminosity, etc. Only very preliminary studies of possible accelerator configurations have been conducted. A preconceptual design conducted by Brookhaven National Laboratory, and based upon the use of existing tunnels, refrigerator, etc. from the ISABELLE project, estimates a construction cost of $150-200 million.

Question 6: Are there alternative concepts, perhaps requiring technological breakthroughs, which would allow access to the multi-TeV energy regime without such a gigantic machine as SSC is currently configured?

Answer: Currently there are no foreseeable alternate concepts which would allow access to the multi-TeV region in the time scale appropriate for the proposed Superconducting Super Collider (SSC). The multi-TeV region can, in principle, be reached with circular hadron-hadron (i.e., proton-proton or proton-antiproton) colliders, or with colliding electron-positron linear accelerators. The scale (size) of the circular devices is determined by the strength of the magnetic field which bends the beam in an arc. We know of no alternative concepts which would provide for magnetic fields of significantly higher values than those being considered as a parameter for cost optimization of the proposed SSC. The technology of linear electron-positron colliders is not sufficiently developed for consideration as a multi-TeV machine option at this time.

Question 7: What are your current estimates of how long it will take; and how much money it will cost to arrive at the point at which you will have a fixed design concept and reasonable estimates of construction costs for the proposed SSC?

Answer: We view the Superconducting Super Collider (SSC) as basically an engineering development challenge building on an existing technical base provided by the Fermi National Accelerator Laboratory (Fermilab) Energy Saver, the Brookhaven National Laboratory (BNL) ISABELLE/Colliding Beam Accelerator (CBA) R&D efforts, and other programs at Department of Energy (DOE) laboratories and at universities. It follows that a success-oriented engineering development plan is appropriate as an organizing principle for Phase I of SSC: i.e., successive narrowing of options leading to design choices based on well defined engineering and economic criteria.

The first phase of the SSC project is expected to take about three-to-four years at a total cost of $150-$200 million. During this period there will be produced a series of conceptual designs and interim cost estimates which are intended to guide the R&D effort and to aid in decision making. A final conceptual design is anticipated at the end of the second year of Phase I which will fix the design concept and provide a reasonable estimate of construction costs.

In parallel with these studies an intensive R&D effort will be conducted to demonstrate experimentally that accelerator components consistent with the conceptual designs can be built with the required performance characteristics, and,to provide a second basis for cost estimation.

Question 8: What are the main problems you need to address in the planning and R&D period before a decision could be made to construct the SSC? Are there any technological or cost breakthroughs necessary in order to make this project viable?

Answer: During the planning and R&D phase of the Superconducting Super Collider (SSC) project we must address the following issues:

- o Define the scope of the facility based on end use related needs.
- o System studies, tradeoff studies, and design efforts to optimize the performance and reliability and to minimize the cost of major subsystems and of the SSC as a whole. In these studies parameters of components such as superconducting magnets, cryogenic systems, and other accelerator systems will be explored which are beyond what has been experimentally demonstrated in the past. A parallel R&D effort to produce prototypes of critical components will be conducted to demonstrate the validity of performance assumptions and costs.
- o Generic site-related issues would be studied and a document which defines site criteria would be prepared and disseminated.
- o A design handbook and formal proposal for construction would be prepared.
- o The methodology for site selection would be developed by DOE.
- o Consideration would be given to the appropriate approach to management of SSC during the construction phase and subsequent operations of the facility.

There are no technical breakthroughs required to make SSC a viable facility.

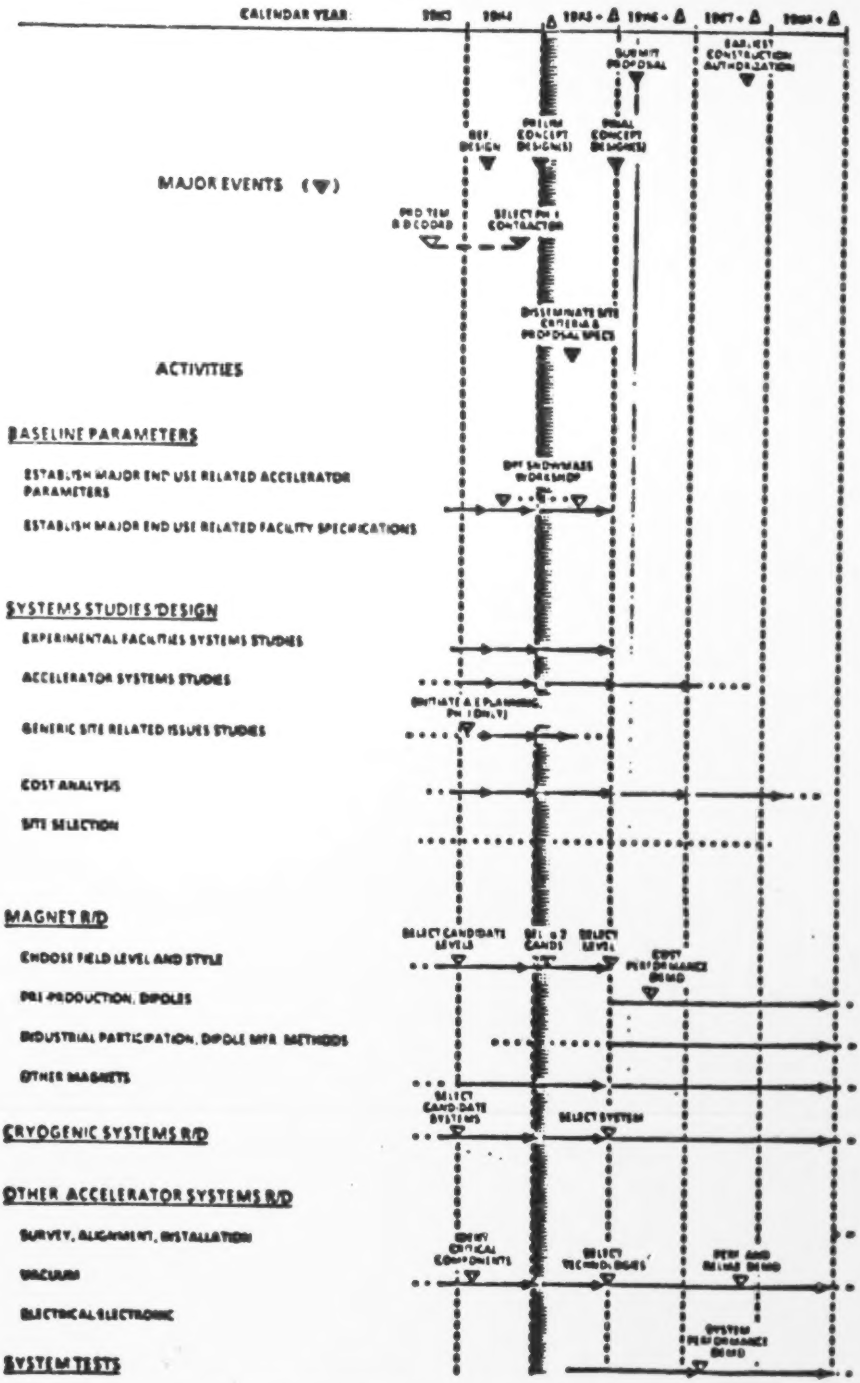
Question 9: Assuming an R&D program and planning for this facility were approved, how do you plan to go about organizing this effort?

- Are sufficient personnel available? Where?
- What kind of organizational structure do you envision?
- What approximate size would you expect an organization to be?
- What is your general timetable for proceeding and what are your major milestones?

Answer:

- Sufficient personnel are available at the various high energy physics laboratories and the universities to support the proposed Phase I effort of R&D and planning. These facilities include Brookhaven National Laboratory, Lawrence Berkeley Laboratory, Fermi National Accelerator Laboratory, Argonne National Laboratory, Los Alamos National Laboratory, Cornell University, and the Stanford Linear Accelerator Center. In addition, a broad-based consortium of university scientists, with support from the laboratories, has expressed a strong interest in participating in the R&D and planning for SSC.
- Possible organizational structures for the Phase I of SSC are the subject of discussion at the present time.
- The R&D Center might initially be staffed by 20-40 full-time people. However, as stated previously, organizational structures are still under discussion.
- An early (perhaps optimistic) preliminary timetable with major events (milestones) is given in the accompanying summary chart.
- The Phase I R&D effort as planned would permit a request for authorization of Phase II for FY 1988, should that be appropriate.

SUMMARY
SSC TECHNICAL, PRE-PROPOSAL, R/D PLANNING GUIDE – Phase I


CALENDAR YEAR:
1983
1984
Δ
1985 + Δ
1986 + Δ
1987 + Δ
1988 + Δ
SUBMIT PROPOSAL
EARLIEST CONSTRUCTION AUTHORIZATION
REF. DESIGN
PRELIM CONCEPT DESIGN(S)
FINAL CONCEPT DESIGN(S)
MAJOR EVENTS (▼)
PRO TEM R/D COORD
SELECT PH I CONTRACTOR
DISSEMINATE SITE CRITERIA & PROPOSAL SPECS
ACTIVITIES
BASELINE PARAMETERS
ESTABLISH MAJOR END USE RELATED ACCELERATOR PARAMETERS
DPF SNOWMASS WORKSHOP
ESTABLISH MAJOR END USE RELATED FACILITY SPECIFICATIONS
SYSTEMS STUDIES/DESIGN
EXPERIMENTAL FACILITIES SYSTEMS STUDIES
ACCELERATOR SYSTEMS STUDIES
INITIATE & E PLANNING, PH I ONLY
GENERIC SITE RELATED ISSUES STUDIES
COST ANALYSIS
SITE SELECTION
MAGNET R/D
SELECT CANDIDATE LEVELS
SEL ≤2 CANDS
SELECT LEVEL
CHOOSE FIELD LEVEL AND STYLE
COST PERFORMANCE DEMO
PRE-PRODUCTION, DIPOLES
INDUSTRIAL PARTICIPATION, DIPOLE MFR. METHODS
OTHER MAGNETS
SELECT CANDIDATE SYSTEMS
SELECT SYSTEM
CRYOGENIC SYSTEMS R/D
OTHER ACCELERATOR SYSTEMS R/D
SURVEY, ALIGNMENT, INSTALLATION
IDENT. CRITICAL COMPONENTS
SELECT TECHNOLOGIES
PERF AND RELIAB DEMO
VACUUM
ELECTRICAL/ELECTRONIC
SYSTEM PERFORMANCE DEMO
SYSTEM TESTS

Note: Δ represents uncertainty in schedule due to unforeseen developments.

Question 10: On page 8 of his testimony, Dr. Wojcicki argued that the SSC should be a hadron-hadron collider, of which Tevatron I will be the prototype, rather than a lepton-lepton collider, of which the SLAC Linear Collider (SLC) at Stanford will presumably be the prototype.

- Is this a correct interpretation?
- Aren't Tevatron I and SLC due to finish the construction phase and begin operation in the 1986-1987 time frame?
- If Tevatron I and SLC operate to currently predicted specifications, won't this open the way to much higher energies for both hadron-hadron colliders and for lepton-lepton linear colliders?
- Won't the increase in energy of Tevatron I to the 10-20 TeV regime be roughly equivalent to the increase in energy of the SLC to the 1-2 Tev regime?
- Might it not be premature to concentrate totally on a 10-20 TeV hadron-hadron collider at this point? Wouldn't it also be wise at this stage to consider a 1-2 TeV lepton-lepton linear collider?

Answer: The High Energy Physics Advisory Panel (HEPAP) Subpanel did conclude that the Superconducting Super Collider (SSC) should be a hadron-hadron collider. This conclusion was based on the fact that the technology of hadron-hadron colliders, including superconducting magnet technology, is believed to be well in hand as a result of the successful operation of the Energy Saver at Fermi National Accelerator Laboratory (Fermilab) and achievements in superconducting magnet development at Fermilab and in the Brookhaven National Laboratory Colliding Beam Accelerator (CBA) R&D program. In the case of lepton linear colliders there are many technological hurdles which must be conquered before linear colliders are proven to be a technically feasible and economical route to the TeV energy range.

Tevatron I is scheduled for completion late in FY 1986 and SLC is scheduled for completion in early FY 1987.

The successful operation of the Fermilab superconducting magnet ring is a major step which opens the way for future higher energy hadron-hadron colliders. To a large extent it is believed that the physics potential of a 10-20 TeV hadron-hadron collider is technology and engineering R&D to reduce the costs of the superconducting magnets and other critical components. The technology of linear electron-positron colliders is not sufficiently developed for consideration as a multi-TeV machine option at this time.

We believe that the decision to proceed rapidly toward a 10-20 TeV hadron-hadron collider is the correct and prudent choice at this time. Since there are no known technological hurdles to achieving an SSC, we are confident that we can design a workable SSC today.

Question 11: What long term effect will the SSC, if constructed, have on the usefulness and longevity of the existing high energy physics accelerator facilities?

Answer: The decision to proceed with the Superconducting Super Collider (SSC) will require that existing high energy physics facilities and the Tevatron and SLAC Linear Collider (SLC) projects, to be completed in the near future, be utilized strongly into the 1990's and upgraded, where appropriate, in order to assure a forefront U.S. high energy physics program. This course of action extends the longevity of some existing facilities compared to a course that would have provided a new facility in the late 1980's. In the early 1990's some existing facilities would be phased out to provide manpower and fiscal resources for development of SSC detectors and preparation for SSC experiments.

Question 12: With regard to the physicists involved in on-going high energy physics activities, what adjustments will be necessary to experimental programs and facility utilization because of the long period of time in which no U.S. facilities will be constructed?

Answer: The U.S. program does have new facilities coming into operation between now and the time the Superconducting Super Collider (SSC) would come into operation. The Tevatron II facilities are scheduled to be completed in phases between now and the end of FY 1985, Tevatron I is scheduled to come into operation late in FY 1986, and the SLAC Linear Collider is scheduled to come into operation in mid-FY 1987. During the period prior to SSC operation, the Department, consistent with the advice from the High Energy Physics Advisory Panel, plans to stress completion of on-going construction and utilization of existing facilities at a high level until the 1990-1992 period when some existing facilities would be phased out to free up manpower and fiscal resources for preparation of SSC detectors and experiments.

Question 13: Siting of a facility such as SSC has already attracted a lot of attention in the media. What are your contemplated procedures for addressing the siting issue?

Answer: We plan that one product of the Phase I R&D program will be a site criteria document which will form the basis for solicitation of site proposals to be considered in a competitive site selection procedure. The timetable of actions and precise procedures have not yet been established.

Question 14: We have heard SSC cost construction estimates ranging from under $2 billion on the optimistic side to $8-$9 billion on the pessimistic side.

- What maximum cost level do you think can be tolerated for the construction of the SSC?
- If Phase I cost estimates of the SSC exceed this maximum level, what alternatives exist?

Answer: A major goal of Phase I of the Superconducting Super Collider (SSC) project is to produce a credible cost estimate for the construction of the SSC based on a cost-optimized design with supporting technology and engineering R&D to achieve substantial cost reduction of in-hand technology and cost-performance demonstrations for major systems such as superconducting magnets. This process will take several years, and so it is premature, at this point, to attempt to identify a likely cost for SSC.

Should the estimated costs of SSC resulting from Phase I design be excessive, we would extend the period of R&D. We are strongly convinced that with sufficient R&D the cost of the facility can be appreciably reduced from that using presently demonstrated technology. While we presently believe that a three-to four-year R&D program will achieve cost-reduction goals and permit early operation of SSC, a longer R&D period might be necessary. At some point, the possible cost-reduction benefit of further R&D must be weighed against the cost of delay of achieving physics benefit from use of the facility.

Question 15: The July 1977 issue of Reviews of Modern Physics contained an article by H. Hahn, M. Month and R. R. Rau titled "Proton-proton intersecting storage accelerator facility ISABELLE at the Brookhaven National Laboratory." On page 627 of this article, it is stated: "Detailed work on the ISABELLE design began at Brookhaven in May 1971." Please provide, by fiscal year, from FY 1971 through FY 1983, the following information for the ISABELLE/CBA project:

- o Operating expenses
- o Capital equipment expenses
- o Construction funding.

Please delineate those funds expended at Brookhaven National Laboratory and those funds expended at other institutions.

Answer: Funding for R&D at Brookhaven National Laboratory (BNL) during the period from FY 1971-1977 was $10.4 million. This included the pioneering work by BNL in developing the technology of superconducting magnets suitable for use in accelerators. It is difficult to specify how much of this funding should be identified as specifically related to the ISABELLE effort. Construction planning and design funds amounted to $1.8 million during these years.

The following table indicates the R&D and construction funding related to the ISABELLE/Colliding Beam Accelerator (CBA) projects at BNL during the years FY 1978-1983.

	(Millions of Dollars)	
	R&D	Construction
FY 1978	$ 5.2	$ 5.0
FY 1979	5.0	23.0
FY 1980	7.0	41.0
FY 1981	15.4	35.0
FY 1982	21.0	15.0
FY 1983	19.9	5.0

In addition to the R&D and construction funding indicated above, BNL spent $3.9 million over the period from FY 1978-83 for capital equipment in support of the R&D effort.

Approximately $1 million was spent at Lawrence Berkeley Laboratory from late FY 1980 through early FY 1982 to design, fabricate and test models of an alternate magnet to be considered for use in CBA.

Question 16: You explained why the Colliding Beam Accelerator (CBA) at Brookhaven National Laboratory is apparently no longer considered a viable facility in the High Energy Physics program.

- What effect does suspension of construction of the CBA at Brookhaven have on the overall high energy physics program in the U.S.?
- What effect does it have on the Brookhaven personnel and programs.

Answer: As noted by the High Energy Physics Advisory Panel (HEPAP), much of the physics that can be done by CBA can be done by Tevatron I at Fermi National Accelerator Laboratory and the European Organization for Nuclear Research (CERN) Antiproton Proton Collider (S̄PPS). Recent developments in physics research point strongly to the 1-2 TeV mass range, which is beyond the capability of the CBA, as the regime in which the next round of fundamental breakthroughs in understanding is anticipated. The substantial fiscal and manpower resources required to construct, equip, and operate CBA would interfere with achieving the SSC on a reasonable time scale within a constrained overall program budget. The physics impetus for exploration in an energy range beyond the capability of CBA, together with the concern over fiscal and manpower resources, form the basis of the decision not to approve CBA.

The decision to not construct the CBA project leads to a significant redirection of the high energy physics program at the Brookhaven National Laboratory (BNL). The CBA R&D groups and the magnet production capability are being redirected to include R&D tasks in support of the SSC project. The magnet production and test facilities at BNL, including the large helium refrigerator, comprise a unique capability. The existence of these facilities, coupled with the acknowledged expertise of the BNL magnet R&D staff, is important for the R&D effort related to the SSC and puts BNL in a position to play a major role in the SSC R&D program.

Question 17: The Subpanel report notes that the SLAC Linear Collider (SLC) at Stanford: a) will have physics objectives similar to those of the LEP machine at CERN in Europe; b) will have a luminosity inferior to that of the LEP; 3) will have one experimental region versus six experimental regions for LEP; and d) will operate in an energy regime already being explored by the CERN SPS collider and which will be explored later by Tevatron I and LEP.

- If these points are reasonably accurate, could it not be argued that the SLC will not be performing at the forefront of high energy physics?
- On physics grounds alone, couldn't one argue that it would be just as logical for the Subpanel to have recommended cancellation of the SLC as the cancellation of CBA?

Answer: It must be recognized that the SLAC Linear Collider (SLC) has a dual purpose--to demonstrate certain aspects of a new accelerator technology which is critical for any economically feasible future very high energy lepton-lepton collider; and to permit U.S. scientists early access to the important physics that can be done with copious production of the Z°.

We believe that SLC will be a forefront research facility. While it is true that SLC will operate in approximately the same energy regime as for the constituent-constituent collisions at the European Organization for Nuclear Research (CERN) Antiproton Proton Collider ($S\bar{P}PS$) and Tevatron I, the physics process being studied is quite different.

While SLC will have only a single interaction region, it will have a significant impact in the area of Z° physics before Europe's LEP (Large Electron Positron) facility can operate. Even after LEP comes into operation, SLC will still be a world-unique facility because it can produce collisions of polarized beams which cannot be achieved at LEP; and the very small size of the SLC beams, together with advanced technology vertex detectors, will permit direct measurement of particle lifetimes more readily than at LEP.

There are substantial qualitative differences between the Colliding Beam Accelerator (CBA) and SLC situations with regard to physics capabilities. The SLC will give U.S. physicists access to the known important realm of physics accessible with copious production of the Z^0 at least two years before LEP could operate, and even after LEP comes into operation, SLC will have world-unique capabilities. The CBA, coming into operation 6 years after the CERN S$\bar{P}$PS collider and one to two years after the Tevatron I collider at Fermi National Accelerator Laboratory (Fermilab), would enter a research realm in which many of the important discoveries will already have been made. CERN has already discovered the W and Z^0 and will almost certainly discover the t-quark.

Question 18: At the Summer 1982 Snowmass meeting of the American Physical Society Division of Particles and Fields, Nobel Laureate Burton Richter of SLAC gave a talk titled, "The Future of Electron-Positron Colliders." During this presentation, he listed beam parameters near those of the SLC for a 1 TeV by 1 TeV electron-positron collider, and noted: "It is certainly not feasible to simply [sic] extend standard linacs like SLAC, for if we did, we could have a total length of machine of about 60 km a power consumption of gigawatts, and a cost of $\$2 \times 10^9$. However, if we can make energy efficient, low cost per unit length accelerators, the world high energy physics community can afford such a machine at its present budget level. We don't know the best way to build such a machine and an intense R&D program will be required to determine the best road."

- Is the SLC project developing energy efficient, low cost per unit length accelerators, or is it relying primarily on existing SLAC linac technology?
- If the SLC is relying primarily on existing SLAC linac technology, then isn't it hard to argue that SLC is really developing the technology of electron-positron linear colliders "which may open the way to much higher energies"?
- On accelerator technology development grounds alone, isn't it hard to argue that the SLC should be built?

Answer: The SLAC Linear Collider (SLC) project uses the existing Stanford Linear Accelerator Center (SLAC) linear accelerator with an energy upgraded to 50 GeV as its injector and does not address the issue of energy-efficient high-gradient accelerating structures. SLC focuses on those issues which are critical to demonstrate technical feasibility of the linear collider concept, such as the formation of very intense micron-sized electron and positron beams, maintaining the properties of these beams through acceleration and beam transport systems, and beam control techniques to provide collisions of adequate luminosity for physics experiments.

SLC provides the essential demonstration of the technical feasibility of linear colliders. Success of the SLC would be a major accelerator technology milestone, even though it does not address all of the issues required for a cost effective TeV-range linear collider system. Without the successful demonstration of the essential technical feasibility of good luminosity which SLC will provide, it would be hard to defend the extensive further R&D which will be required for very high energy linear colliders.

Question 19: Section V.F. of the January 1982 "Report of the Subpanel on Long Range Planning for the U.S. High Energy Physics Program of the High Energy Physics Advisory Panel" (i.e., the Trilling Panel report) includes comments on the individual subsystems and technical areas associated with the SLC project. These include the following:

- Cost and Schedule
- Project Management
- Damping Ring, 1 GeV Buncher Section, and Arc Structure
- SLC Positron Source
- Klystron R&D and Various Energy Options
- Beam-Beam Interaction
- Final Focus
- One-Sector Results
- Buncher-Injector Systems
- SLC R&D Program
- Performance Improvement and Contingency
- Accelerator Physics

Please update the Trilling Panel report comments for each of these areas. Please indicate changes from those comments given in that report, including reasons for these changes.

Answer: The SLAC Linear Accelerator (SLC) project was approved for construction in the FY 1984 budget process, and construction is now underway. Project configuration remains essentially that described in Section V-F of the Trilling Panel report, upon which the comments in Section V-F are based. The major change from that configuration is the decision to construct a single interaction region instead of two. Options remain to add a second interaction region and to increase the energy to 70 GeV on 70 GeV should the physics program require these at some future date.

o Cost and Schedule - First year funding of SLC is authorized at $32 million. The FY 1984 President's budget requested $40 million for the first year (FY 1984) and $50 million and $22 million for the two out years, based on a scheduled completion and first beam capability in the last quarter of FY 1986. The Total Estimated Cost (TEC) for the requested funding profile was $112 million. The reduction from $40 million to $32 million in first year funding will result in some cost increase and delay of the project. The construction of the transport tunnels now underway and the interaction region building remain on the critical path.

- The management structure for the project is in place and functioning well. The project director was selected in August 1982 and has put together an outstanding team of scientific and management experts to supervise the SLC construction and supporting R&D. Approximately $9 million in construction contracts were signed in the first month of FY 1984, reflecting the advanced design status and management preparation.

- Damping Ring, 1 GeV Buncher Section and Arc Structures - The first of the two damping rings required for SLC operation has been completed and is in operation. This ring is unique in design and has been built and is being operated as part of the R&D supporting the SLC. Two major milestones have been accomplished which relate to the damping ring and the associated 1 GeV rf bunches. The invariant emittance of the damping ring has been shown to meet the design specification at the current operating energy (960 MeV) and intensity (10^{10} electrons/bunch) and the buncher has successfully reduced bunch length to the required 1 mm. An active R&D program continues to study the damping ring, buncher and associated beam transport lines. The arc magnet beam transport systems, which guide the 50 GeV electron and positron beams from the end of the linac to the interaction region, have continued to receive in-depth theoretical study. The design is now fixed.

- SLC Positron Source - There is no significant change in the status of this item.

- Klystron R&D - At the time the Trilling report was published, SLAC was considering three options for the energy upgrade of the linac: an expansion of the existing 36 MW system, development of a 50 MW klystron system, and development of a 150 MW system. Subsequent R&D has eliminated the 150 MW option as requiring a longer development time and entailing higher risk than the other alternatives. The 50 MW system with SLED II has emerged as the preferred way to go. The R&D program for the 50 MW system now looks very promising, and it is anticipated that it will be chosen for the linac upgrade.

- Beam-Beam Interaction - There is no significant change in the status of this item.

- Final Focus - The final focus system consists of the optics required to transform and transport the beams from the arc magnet system to exceedingly small transverse cross sections at the interaction point. Although the design is not yet complete, sufficient progress has been made for detailed engineering to be started on most of the elements.

- Linac Sector One Results - The R&D program described in the Trilling report has been successfully completed as far as can be done prior to the installation of additional focusing elements now scheduled for the summer of 1984.

- Buncher-Injector System - This system, which produces 40 MeV bunches of proper emittances and length to enter Sector 1 of the linac, has now met all design specifications. The laser driven polarized electron source has also operated successfully.

- SLC R&D Program - R&D in support of the SLC is progressing well. Milestones are being met on schedule, and the successful operation of the first damping ring and a prototype 50 MW tube are very encouraging.
- Performance Improvement and Contingency, and Accelerator Physics - There are no significant changes in the status of these items.

Question 20: Please answer the following questions concerning the Fermilab Energy Saver Project (Project 79-9-b):

- When was construction of the project completed?
- When did operation of the device begin?
- How does the current performance of the Energy Saver compare with the performance predicted prior to construction?
- What difficulties have been encountered in the initial operation of the Energy Saver, and how are they being overcome?
- Total construction cost for the project were estimated to be $46.6 million, including $28.8 million for magnets, $4.4 million for refrigeration, $1.4 million for power supplies and controls, $0.4 million for rf, $0.1 million for extraction, $2.2 million for special facilities, $0.1 million for conventional facilities, and $6.8 million for contingency. Please provide the final costs for each of these construction cost categories, and explain any variance between the final costs and the estimates.
- Other project funding was estimated to total $52.043 million, including $50.043 million for R&D and $2.0 million for other project related costs. Please provide the final costs for each of these categories, and explain any variance between the final costs and the estimates.

Answer: Formal closeout of the Energy Saver construction project is expected by September 30, 1984.

First accelerated proton beams were obtained June 4, 1983, and a new world record of 512 GeV was attained on July 3, 1983. Operation for physics experiments started October 1, 1983, as scheduled.

All performance specifications have been met with the exception that the top energy to date for circulating beam has been 700 GeV, not the 800 GeV specified as an Energy Saver objective. It is expected that this design objective will be met during FY 1984 as operational experience is accumulated.

The startup of the Energy Saver was free of major problems. There were only minor startup difficulties, as are expected with any complex new accelerator, which were quickly identified and corrected.

In April 1982 the concerned Congressional Committees were informed of the Department's plan for reprogramming to accommodate an increase in the total estimated cost for the Energy Saver to $50.8 million. The Committees expressed no objections. These estimates, and actual expenditures as of September 1983, are (in millions):

	FY 1981 Data Sheet	Estimate 4/20/82	Status as of 9/30/83
Engineering, design and inspection	$ 2.4	$ 2.7	$ 2.4
Magnets	28.8	38.3	37.0
Refrigeration	4.4	5.6	6.1
Power supplies and controls	1.4	1.6	1.4
RF	0.4	0.2	0.3
Special facilities	2.2	1.0	1.0
Conventional facilities	0.1	-	-
Extraction	0.1	-	-
	39.8	49.4	48.2
Contingency	6.8	1.4	2.6
Total estimated cost	$46.6	$50.8	$50.8

The cost increase is due primarily to two major factors, both involving the very high technology superconducting magnets which are the heart of the project and the dominant item in the cost estimate. The first factor is that, during several stages of the magnet fabrication and assembly processes, problems were encountered in fully meeting some of the performance specifications. These problems were subjected to intensive R&D and successfully solved. The second factor is that the degree of quality control required, in the vendor fabrication of various components of the magnets and in the fabrication and assembly of the complete magnets, had to be considerably more intense than foreseen.

The remaining work to be done out of the contingency funds relates primarily to the refrigeration system. Since the static heat load in the superconducting dipole magnets is higher than anticipated, some additional refrigeration is needed in order to have a safety margin for holding all of the Saver magnets at liquid helium temperature during periods when the central helium refrigeration system is shut down for maintenance. This cost, plus the cost of several other required modifications, will be met from the remaining contingency funds of $2.6 million.

Other project funding, as presented to the Congress in the April 1982 revised construction Project Data Sheet, was $68.7 million for R&D and $13.0 million for other project-related costs. As of September 30, 1983, total expenditures were $75.0 million for R&D and $8.4 million for other project-related costs. The additional $6.3 million spent on R&D was occasioned by unexpected technical difficulties encountered in finalizing the engineering design of the Energy Saver superconducting magnets.

Question 21: Please answer the following questions concerning the Tevatron I project (Project No. 81-E-218):

- What is the construction status of the accumulator and debuncher rings?
- What is the construction status of the two experimental-detector areas?
- Is Tevatron still within the estimated construction budget of $82.5 million? If so, is the $13.75 million in contingency still available? If not, what explanation is there for additional cost overruns? Is the project on schedule?
- Have any technical difficulties been encountered with magnet fabrication and performance, refrigeration upgrade, or rf cavity fabrication? If so, please describe the difficulties and corrective measures being taken.
- Are the direct R&D operating costs within the $38.968 million estimated? If not, please explain the reasons for any variances.
- What major items of equipment are included in the $2.889 million capital equipment budget? Will all the required capital equipment be provided for $2.889 million, or will aditional funding be needed and if so, how much and for what reason?

Answer: The accumulator and debuncher rings are about 20 percent complete, taking into account the status of both technical components and conventional construction.

Civil construction of the first experimental area is complete, and design of the second area is underway with construction expected in 1985.

The total estimated construction cost for the project remains at $82.5 million, with uncommitted contingency to date of about $12 million. The project is now about four months behind schedule because the final design of the debuncher lattice configuration took longer than anticipated and thereby delayed the start of civil construction on the antiproton source ring enclosure. This work is now progressing well, and we anticipate no slippage of the FY 1986 project completion date.

Minor difficulties were encountered in the design of magnets for the accumulator and debuncher rings, due to the stringent tolerances required for their performance in the antiproton beam cooling systems. This caused a delay of about two months in magnet fabrication, but that is now progressing well. No overall impact on the project schedule is foreseen. No technical difficulties in the refrigeration upgrade or rf cavity fabrication have been encountered.

The R&D costs for the project are now estimated at $43.4 million; an increase of about $4.3 million. This is due to two major items whose costs were underestimated; four reference magnets against which all production magnets are to be compared, and a proton transfer line from the existing booster accelerator to the debuncher ring which will permit studies of the antiproton source rings without interference with operation of the Energy Saver for the Fermi National Accelerator Laboratory physics research program.

Of the roughly $1.5 million spent to date on capital equipment in support of the Tevatron I R&D programs, the major items purchased are very high frequency electronics for the stochastic cooling studies and a data acquisition computer and related equipment needed to develop and measure the prototype and reference magnets. No additional capital equipment funding in support of the R&D effort beyond the $2.889 million budgeted is expected to be required. As indicated in the Construction Project Data Sheet for this project, this category does not include the funding for the major detectors.

Question 22: Please answer the following questions concerning the Tevatron II project (Project No. 82-E-206):

- What is the construction status of the extraction system?
- What progress has been made in upgrading the external beam switchyard to 1 TeV?
- What is the status of the 1 TeV target assembly and experimental stations?
- What is the status of the new secondary beam lines (and beam line replacements) in the external experimental areas? What percentage of the equipment has been completed and what progress has been made on the support facilities?
- Will Tevatron II construction be completed within the original schedule for $49.0 million or less? If not, please explain the reasons for any expected schedule and/or cost overruns.
- If the project is still within budget, how much of the $7.8 million contingency remains uncommitted? How much of the other project-related costs and direct R&D of the estimated $18.0 million remains uncommitted?

Answer: The extraction system is complete and in routine operation.

Upgrading of the external beam switchyard is complete and in operation, except for the components transferring primary protons into the new muon beam line which are scheduled to be completed in FY 1984.

Of the eleven 1 TeV capability primary target stations in the project, seven have been constructed and two of these are currently in use.

Designs for the new secondary beam lines have been finished, and the procurements and fabrication on special facilities are about 30 percent complete. Civil construction has started on two experimental halls, with the remaining two scheduled to be started in the fall of 1984. Construction of the support facilities is about 50 percent complete.

The construction cost estimate for Tevatron II remains $49.0 million. We expect that the project will be completed within budget and on schedule.

Uncommitted Tevatron II construction contingency is now $5.0 million. Approximately $7.5 million of direct R&D and other project related costs remain to be committed mostly in FY 1984.

Question 23: Please provide 1) a listing of all reports by the High Energy Physics Advisory Panel since its establishment in January 1967; and 2) two copies of each of these reports.

Answer: Following is a listing of all reports by the High Energy Physics Advisory Panel (HEPAP) since its establishment in January 1967.

1. July 1983, Report of the 1983 HEPAP Subpanel on New Facilities for the U.S. High Energy Physics Program (DOE/ER-0169)
2. January 1982, Report of the Subpanel on Long Range Planning for the U.S. High Energy Physics Program of the High Energy Physics Advisory Panel (DOE/ER-0128)
3. November 1981, Interim Report of the Subpanel on Long Range Planning for the U.S. High Energy Physics Program of the High Energy Physics Advisory Panel (DOE/ER-0112)
4. July 1980, High Energy Physics Advisory Panel, Report of the 1980 Subpanel on Review and Planning for the U.S. High Energy Physics Program (DOE/ER-0066)
5. June 1980, Report of the Subpanel on Accelerator Research and Development of the High Energy Physics Advisory Panel (DOE/ER-0067)
6. October 1978, Review of the Future of the High Energy Physics Program at the Argonne National Laboratory (DOE/ER-0017)
7. June 1978, Report of the Subpanel on High Energy Physics Manpower of the High Energy Physics Advisory Panel (DOE/ER-0010)
8. April 1978, Report of the Subpanel on Computing Needs of the High Energy Physics Advisory Panel (DOE/ER-0007)
9. June 1977, Report of the 1977 Subpanel on New Facilities of the High Energy Physics Advisory Panel (ERDA 77-71)
10. July 1975, Report of the 1975 Subpanel on New Facilities of the High Energy Physics Advisory Panel (ERDA-83)
11. October 1974, Report prepared by the Subpanel on the Health of High Energy Physics of the High Energy Physics Advisory Panel
12. July 30, 1974, Report of the Subpanel on New Facilities of the High Energy Physics Advisory Panel
13. December 1973, Possibilities for US-USSR Cooperative Efforts in Research on Fundamental Properties of Matter

14. June 1972, Future Patterns of High Energy Physics Research

15. September 1971, Advanced Accelerator Concepts and Technology

16. August 1970, Computer Usage in High Energy Physics

17. June 1969, Report on High Energy Physics

18. January 1968, The Status and Problems of High Energy Physics Today

19. January 1968, Report in Response to Questions Pertaining to the Scope of the 200 BeV Accelerator

Two copies of each of the above reports are attached.

[Reports listed as 1, 2, 3, 4, 5, 9, 10, 11 and 12 are reproduced in Appendix II.]

DON FUQUA (Fla.), Chairman

ROBERT A. ROE, N.J.
GEORGE E. BROWN, JR., CALIF.
JAMES H. SCHEUER, N.Y.
RICHARD L. OTTINGER, N.Y.
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ROBERT G. TORRICELLI, N.J.
FREDERICK C. BOUCHER, VA.

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J. H. POORE
Executive Director
ROBERT C. KETCHAM
General Counsel
DAVID S. JEFFERY
Minority Staff Director

October 27, 1983

Stanley G. Wojcicki, Chairman
Department of Physics
Stanford University
Stanford, California 94305

Dear Dr. Wojcicki:

Thank you for your excellent testimony before the Subcommittee on Energy Development and Applications on October 19, on the Department of Energy's high energy physics facilities. Attached are some questions that Members of the Subcommittee would like you to answer, in order to supplement the record of the hearing on this topic.

I would appreciate your responses to these questions by November 15, 1983. If you have any questions regarding this request, please contact Dr. Harlan Watson of the staff of the Subcommittee on Energy Development and Applications at (202) 225-4320.

Sincerely,

DON FUQUA
Chairman

DF:Wcw
attachment

Additional Questions for Dr. Stanley G. Wojcicki

1. On page 8 of your testimony, you argue that the SSC should be a hadron-hadron collider, of which Tevatron I will be the prototype, rather than a lepton-lepton linear collider, of which the SLAC Linear Collider (SLC) at Stanford will presumably be the prototype.

    - Is this a correct interpretation?
    - Aren't Tevatron I and the SLC due to finish the construction phase and begin operation in the 1986-1987 time frame?
    - If Tevatron I and SLC operate to currently predicted specifications, won't this open the way to much higher energies for both hadron-hadron colliders and for lepton-lepton linear colliders?
    - Won't the increase in energy of Tevatron I to the 10-20 TeV regime be roughly equivalent to the increase in energy of the SLC to the 1-2 TeV regime?
    - Might it not be premature to concentrate totally on a 10-20 TeV hadron-hadron collider at this point? Wouldn't it also be wise at this stage to consider a 1-2 TeV lepton-lepton linear collider?

2. The Subpanel report notes that the SLAC Linear Collider (SLC) at Stanford: a) will have physics objectives similar to those of the LEP machine at CERN in Europe; b) will have a luminosity inferior to that of the LEP; c) will have one experimental region versus six experimental regions for LEP; and d) will operate in an energy regime already being explored by the CERN SPS collider and which will be explored later by Tevatron I and LEP.

    - If these points are reasonably accurate, could it not be argued that the SLC will not be performing at the forefront of high energy physics?
    - On physics grounds alone, couldn't one argue that it would be just as logical for the Subpanel to have recommended cancellation of the SLC as the cancellation of the CBA?

3. At the Summer 1982 Snowmass meeting of the American Physical Society Division of Particles and Fields, Nobel Laureate Burton Richter of SLAC gave a talk titled, "The Future of Electron-Positron Colliders." During this presentation, he listed beam parameters near those of the SLC for a 1 TeV by 1 TeV electron-positron collider, and noted: "It is certainly not feasible to simple [sic] extend standard linacs like SLAC, for if we did, we could have a total length of machine of about 60 km,

a power consumption of gigawatts, and a cost of \$2 x 10^9. However, if we can make energy efficient, low cost per unit length accelerators, the world high energy physics community can afford such a machine at its present budget level. We don't know the best way to build such a machine and an intense R&D program will be required to determine the best road."

- Is the SLC project developing energy efficient, low cost per unit length accelerators, or is it relying primarily on existing SLAC linac technology?

- If the SLC is relying primarily on existing SLAC linac technology, then isn't it hard to argue that SLC is really developing the technology of electron-positron linear colliders "which may open the way to much higher energies"?

- On accelerator technology development grounds alone, isn't it hard to argue that the SLC should be built?

4. What is your reaction to the Department of Energy's proposal to divert \$5 million of high energy physics funding to modify Brookhaven's high energy physics facilities "to accomodate acceleration of heavy ions"?

   - Might not this lead to the initiation of the construction of a relativistic heavy ion collider, and might not this interfere with the SSC project in a manner similar to the CBA?

   - Would you be concerned about Brookhaven's commitment to a vigorous SSC project if many of the scientists there were distracted by the heavy ion collider project?

   - Do you believe that Brookhaven can remain a major player in the SSC project if it moves forward with a relativistic heavy ion collider project?

STANFORD UNIVERSITY
STANFORD, CALIFORNIA 94305

DEPARTMENT OF PHYSICS

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COMMITTEE ON SCIENCE
AND TECHNOLOGY

November 4, 1983

Hon. Don Fuqua, Chairman
Committee on Science and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Mr. Fuqua,

Enclosed you will find my responses to the questions presented in your letter of October 17, 1983.

Once again, please let me thank you and your committee for this opportunity to present my views on the future of high energy physics in the United States. I shall be happy to provide any additional information that you might consider helpful.

Sincerely Yours,

Stanley Wojcicki
Chairman, 1983 HEPAP
Subpanel on New Facilities

cc:
Professor J. Sandweiss
Dr. A. Trivelpiece

SW:pd
enclosure

RESPONSES TO THE QUESTIONS FROM THE SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS

BY STANLEY G. WOJCICKI

1. It is certainly true that just as Tevatron is a necessary step towards a multi-TeV hadron hadron collider so also SLC is a necessary intermediate step before proceeding to a 1-2 TeV e^+e^- collider.

Both Tevatron I and SLC are scheduled to commence physics operation in the 1986-1987 time frame but there are important differences as to what will be learned before that time. Tevatron I is a project to construct a hadron hadron collider in the 1 TeV energy range by using counter-rotating proton and antiproton bunches in a superconducting ring. Thus the technical questions can be divided into 2 parts:

a) the problems associated with the antiproton source

b) the questions relevant to the superconducting ring of magnets.

I would argue that the feasibility of both of these aspects has been demonstrated already: the existence proof for the $\bar{p}p$ collisions has been given by the operation of the SPS at CERN in the $\bar{p}p$ collider mode; the questions associated with the feasibility of a large superconducting magnet ring have been answered by the present successful operation of the Tevatron ring in the fixed target mode (Energy Saver and Tevatron II project). Furthermore many of the tests required for the multi-TeV, high luminosity collider can be performed now with the Tevatron ring.

I believe that the situation is different with the SLC collider. The basic problems with producing, accelerating, and colliding micron size bunches of electrons and positrons represent a totally new technical domain. Thus, there is a strong possibility that unexpected surprises and problems will arise and the technical feasibility of this technique will not be definitely proven until the beginning of 1987.

Regarding the 3rd and 4th points, I certainly agree that the Tevatron and SLC will lead hopefully to much higher energies for both lepton-lepton and hadron-hadron colliders. The energy increase between a 10-20 TeV hadron collider and the Tevatron I (a factor of 10-20) is comparable but somewhat smaller than extrapolation of SLC to a 1-2 TeV e^+e^- collider (a factor of 20-40).

I believe, however, that there are more complex issues involved here and thus do not think that we can consider a 1-2 TeV lepton-lepton linear collider at this time. There are several technical reasons for this statement, but in essence they can be summarized by saying that whereas the extrapolation from the Tevatron to 10-20 hadron-hadron collider is relatively straightforward, that is not the case for the linear colliders.

I mention only briefly some of the difficulties in extrapolating SLC project to the 1-2 TeV domain:

(a) to achieve required luminosity the size of the bunches would have to be decreased by another order of magnitude. In parallel, one would need corresponding improvements in the alignment techniques and feedback technology.

(b) it is not known how one would achieve the required high luminosity in more than 1 interaction region.

(c) more efficient klystrons and modulators would probably have to be developed or else the power costs would be prohibitive.

Finally, one can make physics arguments in favor of a hadron hadron collider as the optimum machine in the new energy regime. These arguments basically rest on the fact that in general hadron machines tend to be more versatile. They probe a wider variety of possible couplings and thus are more likely to discover new phenomena. Because of the composite structure of the hadrons, the physics productivity of a hadron machine is relatively insensitive to the precise value of the energy of the collider in contrast to the e^+e^- machines, whose physics output can be a rather sensitive function of that parameter.

Thus to summarize my answer to question 1:

(a) the hadron technology is several years ahead.

(b) at the present time the extrapolation from SLC to 1-2 TeV linacs is beset with many uncertainties.

(c) there are physics arguments that argue for hadron hadron collider as a preferable option.

2. I believe that the 4 points mentioned in the introduction to this question are reasonably accurate but need to be elaborated on. I would like first to do this and then address myself to the specific questions:

(a) Both SLC and LEP machines have similar physics objectives, i.e., study of the Z^0 boson and of the e^+e^- annihilations in this general energy region. However, SLC does have some unique features which make physics objectives not completely identical. I will return to the specifics of that point when answering the questions.

(b) It is true that the peak design luminosity of LEP is higher than that of SLC. However there are enough uncertainties associated with that parameter that it is difficult to predict how much higher it will be in reality. In addition we must also take cognizance of the following points:

(i) In a circular machine, because of finite beam lifetime, the average luminosity is lower by a factor of 2 or so from the peak luminosity. That attrition factor will be absent in the linear machine.

(ii) Because of high power consumption the present plans call for LEP to run less than 50% of the available calendar time.

(iii) A machine with several interaction regions is generally less efficiently used than one with only one experimental area. There are invariably losses associated with access required due to detection apparatus failures.

(c) At present there are plans to utilize only 4 interaction regions at LEP.

I doubt that the other 2 will be used in the foreseeable future. Furthermore, in my opinion, the experience of PETRA and PEP would argue that the physics productivity of an e^+e^- machine does not rise linearly with the number of interaction regions.

(d) The energy range of SLC will be comparable to that of SPS collider and Tevatron I, but for the study of the Z°, SLC will offer a much cleaner environment and hence will allow one to explore quite different physics questions. Regarding comparison with LEP, one should bear in mind that SLC is scheduled to start operations some 2 years before LEP completion.

I shall now address the 2 specific questions. From the points I made above, I believe that it follows immediately that in my opinion the SLC will address the forefront high energy physics questions, provided that its time scale and performance are as advertised. More specifically, it will have the opportunity for 2 years to study without any competition some of the questions that the $\bar{p}p$ colliders will not be able to answer.

Regarding the second question, I would like to indicate several reasons why the physics arguments for the SLC are considerably stronger than for the CBA. Some of those points I have alluded to previously, but it might be worthwhile to mention them explicitly again.

1. unlike CBA, the SLC is scheduled for completion about 2 years before its European competitor, LEP. If one adds in the fact that the SLC will commence its operation with a proven and debugged detector, Mark II, the effective lead could easily be about 3 years.
2. even though CBA would provide additional field data on the superconducting accelerator technology, these data would not be unique, but merely complement the Fermilab Tevatron information. In contrast, the SLC experience will be completely unparalleled.
3. there exist several groups ready and eager to commence experimentation at the SLC the moment it turns on. On the other hand, the Woods Hole

subpanel expressed serious doubts whether sufficient manpower and detectors would be available at CBA turn on to properly exploit that machine. This is especially true in light of the complex detection apparatus required for the proper use of the CBA.

4. the SLC holds at least two very important physics advantages over the LEP machine, i.e.
   - (a) it will be relatively straightforward to do polarized beam experiments. This capability is difficult, or maybe impossible in circular machines.
   - (b) the small beam size of the SLC device will allow a much closer positioning of the detectors to the point of interaction. This feature will be very important in the study of short lived states, a projected major area of experimentation both at SLC and LEP.

3. The three subquestions asked here are somewhat related so I would like to answer them together. I believe that the problem of high energy linacs can be separated into two distinct parts, i.e.

- (a) demonstration of feasibility of producing, accelerating, and colliding micron (or sub-micron)-size bunches of electrons and positrons.
- (b) performing the above task in the most economical, i.e. most cost-efficient and energy-efficient manner.

The primary goal of the SLC project is to demonstrate the first aspect of this problem. In the case of the hadron-hadron collider the equivalent task was to demonstrate the feasibility of the $\bar{p}p$ collisions at sufficiently high rate and of the large scale superconducting accelerator technology. I have argued in my answer to question (1) that these two goals have been already achieved.

The hadron-hadron equivalent of point (b) is the development of production techniques for relatively cheap and reliable superconducting magnets for the SSC. This would be one of the main goals of the phase 1 of the SSC R&D program if that project is given the go-ahead by the Federal Government. I believe that in the case of the linear collider the same two-step process is appropriate, i.e. first prove the feasibility (task of the SLC) and secondly find the ways to bring the costs down and improve the reliability of the individual components.

Some new component technology is indeed being developed as part of the SLC project. However, since that technology is not essential to the success of that program, it does not and probably should not receive as high a priority as the main goal of the program, i.e. achieving e^+e^- collisions at the high enough rate and at the interesting energy.

I believe that this always has been the philosophy of the SLC project. One of the main reasons that it can be relatively cheap and proceed on a fast time scale, is because it builds on and utilizes a lot of the existing SLAC linear technology.

4. I must confess to a certain amount of ignorance on the subject of high energy heavy ion collisions. However, the problems of quark and gluon confinement are very basic to our understanding of matter, and any reasonable methods to explore these questions should be pursued. Thus, as a high energy physicist, I am excited by this potential new window on this important problem and my confidence in the fruitfulness of this approach is strengthened by the respect that I hold for some of my colleagues who advocate this particular line of attack. Much as I hate to lose $5 million of funding to my own discipline, I believe that in the overall physics picture the decision does make sense.

I shall turn now to the three more specific questions. If the physics arguments for relativistic heavy ion collider do hold up under closer scrutiny, I would support the construction of such a machine in the CBA tunnel. Such a project would provide not only a unique tool in the whole world with a potential for important new discoveries but also because of the existence of a lot of the required facilities at BNL (CBA tunnel, AGS, etc.) it would be also probably quite cost-effective. I expect that this would be a smaller scale project than CBA, would be done over a slightly longer time period, and hopefully would represent at least a 50% participation by the nuclear physics community. Thus its negative impact on the SSC would be significantly diminished.

Regarding the second question, I would hope that a lot of Phase 0 and Phase 1 work on the SSC could be utilized for the heavy ion collider project. Certainly R&D on the magnets required for the SSC would be also beneficial to the heavy ion collider project. I could even imagine a possible scenario where the pilot-plant produced SSC magnets might be used for that program. Alternatively, the work being done on the superferric magnets as a part of the SSC R&D program could produce the ideal version of the magnet for the heavy ion collider. In brief the needs of the heavy ion collider and the SSC for the first few years will be somewhat complementary; where they are orthogonal, the conflicts can be hopefully alleviated in a large part by an infusion of people from the nuclear physics community.

If the SSC project is to be completed on a reasonably fast time scale, I think it is essential that it build up its managerial and technical staff in the next few years. Hopefully the site for this potential new accelerator could also be chosen during the next 3 or 4 years. By that time most of the R&D would be terminated and involvement by all the National Laboratories (as institutions) would be more specialized, i.e. as subcontractors responsible for specific production tasks. That mode of operation would probably interfere much less with a local construction project like the heavy ion collider.

In summary, the resources of the high energy community are clearly limited. I believe that the SSC should be the top priority item, but that commitment probably does not exclude construction of a heavy ion collider on a reasonable time scale if support from the nuclear physics community will be forthcoming. Under those conditions, I believe that the BNL can contribute significantly to the SSC and still construct the heavy ion collider. Of course, should BNL become the site for the SSC, then the conflicts would become very serious.

DON FUQUA (Fla.), Chairman

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U.S. HOUSE OF REPRESENTATIVES
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General Counsel
DAVID S. JEFFERY
Minority Staff Director

October 27, 1983

Professor Jack Sandweiss
Department of Physics
Yale University
New Haven, Connecticut 06511

Dear Professor Sandweiss:

Thank you for your excellent testimony before the Subcommittee on Energy Development and Applications on October 19, on the Department of Energy's high energy physics facilities. Attached are some questions that Members of the Subcommittee would like you to answer, in order to supplement the record of the hearing on this topic.

I appreciate your cooperation on this matter. If you have any questions regarding this request, please contact Dr. Harlan Watson of the staff of the Subcommittee on Energy Development and Applications at (202) 225-4320.

Sincerely,

DON FUQUA
Chairman

DF:Wcw
attachment

Additional Questions for Dr. Jack Sandweiss

1. If the SSC costs $2 billion to $4 billion, then that is the equivalent of 4 years to 8 years at the current budget level.

    - Is the high energy physics community willing to retire any of its current operating facilities during the years of construction to help finance this project?

    - What would be the most orderly and logical retirement schedule for each of the existing facilities?

2. It may well be that because of its size, the SSC will not be sited at an existing high energy facility. If this is the case, can we expect the termination of one or more of the existing facilities?

3. We have heard SSC cost construction estimates ranging from under $2 billion on the optimistic side to $8-$9 billion on the pessimistic side.

    - What maximum cost level do you think can be tolerated for the construction of the SSC?

    - If Phase I cost estimates of the SSC exceed this maximum level, what alternatives exist?

4. Is the high energy physics community interested in a relativistic heavy ion collider at this point?

5. The SSC R&D and construction phases, if approved by Congress, will be a very long-term effort. What role does the high energy physics community intend to take to assure the Congress that a continuing commitment will be forthcoming?

Responses to the Questions from the Subcommittee on Energy Development and Applications

by Jack Sandweiss

1. If the SSC costs $2 billion to $4 billion, then that is the equivalent of 4 years to 8 years at the current budget level.

   Is the high energy physics community willing to retire any of its current operating facilities during the years of construction to help finance this project?

   What would be the most orderly and logical retirement schedule for each of the existing facilities?

In the past, the U.S. High Energy physics research program has retired older accelerator facilities as their scientific potential has been replaced and extended by newer facilities. For example, the Cambridge Electron Accelerator, the Princeton-Pennsylvania accelerator, the Argonne Zero Gradient Synchrotron and the Brookhaven Cosmotron are all facilities which were phased out as part of the planned evolution of the program. We can certainly expect this general practice to continue in the future.

Turning to the present situation, it appears that the complement of accelerator facilities, currently operating or under construction, have substantial scientific potential. It will be very important to support these facilities during the R and D and construction phases of the SSC, both for the research results which will be obtained and for the health of the field in the United States.

Looking ahead to the time when a substantial effort will be needed to prepare for experiments at the SSC, it does seem likely to me that some of the older facilities may be phased out. In my opinion, it is too early to speculate now as to which these might be.

2. It may well be that because of its size, the SSC will not be sited at an existing high energy facility. If this is the case, can we expect the termination of one or more of the existing facilities?

It seems likely that the SSC, even if located at a presently existing laboratory, will represent a shift of high energy physics effort and so will have a major impact on high energy physics effort and facilities at these laboratories. As in the case of question 1, it seems too early to speculate now as to the exact character of the effects.

3. We have heard SSC cost construction estimates ranging from under $2 billion on the optimistic side to $8-$9 billion on the pessimistic side.

What maximum cost level do you think can be tolerated for the construction of the SSC?

If Phase 1 cost estimates of the SSC exceed this maximum level, what alternatives exist?

I don't really know how to answer the first part of the question. From the point of view of the ability of the high energy physics program to properly manage the construction and utilization of the SSC even the pessimistic number is tolerable. However, from the point of view of national priorities it may well not be. It should, perhaps, be emphasized that the pessimistic number was deduced assuming that _no_ R & D was carried out to reduce the cost.

It is likely that the most significant factor in the cost will be the amount of time and effort expended in the R and D program. We have estimated that three to four years of R and D will be sufficient to realize the cost reductions which are potentially available within our recently acquired, superconducting technology. It may happen that more time and effort than that will be required for that goal.

4. Is the high energy physics community interested in a relativistic heavy ion collider at this point?

The interest in a relativistic heavy ion collider arises in part from discoveries made in high energy physics and from theoretical ideas (quantum chromodynamics) which were developed in the course of high energy research. It also arises very naturally in the evolution of nuclear physics. Indeed, the fundamental insight into the

4. Continued

subatomic many body problem potentially provided by quantum chromodynamics and the recognition of the basic quark, gluon composition of nucleons have led to exciting new directions in nuclear physics. The observation and study of a new state of matter, the quark gluon plasma, which can be anticipated at such a heavy ion collider would be of great interest to particle physicists, nuclear physicists and to physicists working in many body problems generally.

There is also a good possibility that the SSC R & D will have application to the design and construction of an economical relativistic heavy ion collider. Finally, the interest of the nuclear physics community presumably means that a substantial fraction of the cost of such a collider will come from nuclear physics resources. For these reasons, there is interest in the high energy physics community in the construction of such a high energy heavy ion collider. Thus, although the first priority for new facilities in high energy physics is clearly the SSC, there is a feeling in the high energy community that a relativistic heavy ion collider would be sufficiently interesting to both nuclear physics and particle physics and sufficiently consistent with the SSC R and D goals to warrant support.

5. The SSC R&D and construction phases, if approved by Congress, will be a very long-term effort. What role does the high energy physics community intend to take to assure the Congress that a continuing commitment will be forthcoming?

The SSC would open a frontier in the study of the fundamental nature of matter and energy which we cannot begin to approach in any other way in the next 10 to 20 years. It is this fact which lies behind the high energy physics community's unanimous support for the SSC. By recommending that the Brookhaven CBA, which would have been a useful and productive facility, not proceed, the high energy community has clearly made its commitment to the importance of proceeding to the new energy range of the SSC as soon as possible.

The SSC would be a unique facility worldwide. The only other conceivably comparable project would be a hadron collider in the LEP tunnel at CERN. However, the SSC energy is significantly greater than that

5. Continued

which is possible in the LEP tunnel. The magnitude and engineering challenge of the SSC are such that it is not credible that another effort, which is not now known to us, will appear on a time scale to be competitive with the SSC.

The high energy community is broadly engaged in the SSC effort. At present this takes the form of studies of facility specifications and conceptual designs of experimental apparatus. These activities will certainly continue and grow. The design, development and construction of the actual experiments will engage a very substantial segment of the high energy community for many years before the initial operation of the facility. In summary, the scientific promise and the unique character of the SSC assure the long term commitment of the high energy community for its successful realization and use.

APPENDIX II

ADDITIONAL MATERIAL SUBMITTED FOR THE RECORD

THE SECRETARY OF ENERGY
WASHINGTON, D.C. 20585

October 18, 1983

RECEIVED
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COMMITTEE ON SCIENCE
AND TECHNOLOGY

Honorable Don Fuqua
Chairman, Committee on Science
and Technology
House of Representatives
Washington, DC 20515

Dear Mr. Chairman:

This letter is to inform you of the decision by the Department of Energy not to approve the Colliding Beam Accelerator (CBA) project at the Brookhaven National Laboratory (BNL). The CBA project is a proposed successor to the ISABELLE project which was initially, partially authorized by Congress in FY 1978.

As a result of both technical and management difficulties with the ISABELLE project, the project cost and time schedules were increased substantially beyond the original plan. In view of this situation, the Department instructed Brookhaven in FY 1982 to revert to an R&D mode in which they would concentrate on solving the technical and management problems and on examination of a possible new facility that was scientifically important and could be built at a lower cost. In April 1983, BNL submitted a proposal for the CBA which, in scope and performance, is similar to the ISABELLE project. The Department carefully reviewed the CBA proposal and sought the advice of the High Energy Physics Advisory Panel (HEPAP) on whether proceeding with the CBA was the proper course of action for the U.S. high energy physics program from a scientific point of view.

The Department has received a recommendation from HEPAP that the CBA project not be approved. Reviews of the CBA proposal by the Department and by HEPAP have concluded that the facility could be built at the cost and on the schedule proposed by Brookhaven. In recommending that the CBA facility not be built, HEPAP noted that even though there is significant probability of important new physics discoveries at CBA, the existing CERN SPS collider and the Tevatron I collider, which is under construction at Fermilab, will explore much of the physics accessible to the CBA. HEPAP concluded that significantly higher priority should be given to the study of interaction of particles at energies far above the capability of the CBA, in an energy region where there are no existing or planned facilities in the world. This scientific emphasis on the importance of attaining the highest possible energy, together with concern about the optimum use of limited fiscal and manpower resources, forms the basis of the HEPAP recommendation not to approve CBA.

After carefully reviewing the recommendations from HEPAP, the Department has decided not to approve the CBA and to terminate the ISABELLE project. We are requesting Brookhaven to develop a plan for orderly close out of the ISABELLE construction activities and for the protection of already-built facilities using remaining unobligated construction funds from FY 1983 and prior years. We will forward to you, in the near future, a plan for redirection of the $5 million of construction funds appropriated for ISABELLE in FY 1984 to an improvement of the Alternating Gradient Synchrotron (AGS) to provide a new capability for heavy ion research.

Both the Department and HEPAP recognize and acknowledge that the R&D done by BNL in solving their technical problems has contributed substantially to the national capability in superconducting magnet technology. The technical expertise and facilities at BNL will be extremely valuable assets for the advanced accelerator R&D efforts required for future accelerators. The Department plans to assign BNL a major role in FY 1984 in the R&D for a future high energy physics facility.

The FY 1984 funding for BNL accelerator R&D, which had been planned to focus on the CBA project, will be redirected to include advanced accelerator R&D at BNL and elsewhere for a future high energy physics facility, increased R&D related to AGS improvements, and costs for close out of those R&D activities of specific value only to CBA. Related to the redirection of CBA R&D efforts, there will be a net reduction of $7.9 million in the High Energy Technology portion of the BNL operating budget for FY 1984. Consistent with the recommendation by HEPAP, we are providing some additional funding to improve the operating level of the AGS at Brookhaven.

We would be pleased to provide any further information that you may desire.

Sincerely,

Donald Paul Hodel
DONALD PAUL HODEL

cc: Honorable Larry Winn, Jr.
Ranking Minority Member

REPORT OF
THE 1983 U.S. DEPARTMENT OF ENERGY HEPAP SUBPANEL ON NEW FACILITIES FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM

JULY 1983

Yale University New Haven, Connecticut 06511

PHYSICS DEPARTMENT
P.O. Box 6666
260 Whitney Avenue

July 12, 1983

Dr. Alvin W. Trivelpiece
Director, Office of
Energy Research
U.S. Department of Energy
Washington, DC 20585

Dear Dr. Trivelpiece:

I am writing to transmit the report of the 1983 HEPAP Subpanel on New Facilities and to summarize the discussion and conclusions of the HEPAP review of the report which took place on July 11 and 12, 1983. I would like to take this opportunity to thank the Chairman, Dr. Wojcicki, and the Subpanel members for their arduous and important labors.

HEPAP unanimously endorses all of the Subpanel's recommendations:

"1. The Subpanel unanimously recommends the immediate initiation of a multi-Tev high-luminosity proton-proton collider project with the goal of physics experiments at this facility at the earliest possible date.

2. The Subpanel recommends the rapid completion of current construction projects, Tevatron and the SLAC Linear Collider (SLC), the upgrading of the Cornell Electron Storage Ring (CESR) and associated detectors and the thorough utilization of all existing facilities.

3. The Subpanel recommends that Fermilab not proceed at this time with the Dedicated Collider.

4. By a majority vote, the Subpanel recommends that the Colliding Beam Accelerator (CBA) project at Brookhaven not be approved.

5. The Subpanel recommends that technology research and development, particularly advanced accelerator research and development, be strongly supported."

The first and unanimous recommendation of the Subpanel for the immediate initiation of a project aimed at the design and construction of a multi-TeV, high-luminosity proton-proton collider is enthusiastically endorsed by HEPAP with the highest priority. This ambitious project, called the Superconducting Super Collider (SSC), to provide between 10 and 20 TeV per beam, has fired the imaginations of high energy physicists everywhere. The SSC would be the forefront high energy facility of the world and is essential for a strong and highly creative United States high energy physics program into the next century.

HEPAP realizes that this project demands effort and resources unprecedented for a single tool in basic research. It would constitute a new national commitment that should signal the intention of the U.S. to strengthen its support of basic science in all its aspects. As a community of high energy physicists, we would not wish this project to have a negative impact on support of other branches of science.

Experimental discoveries of the past 15 years and their theoretical interpretation have brought a wide ranging synthesis of the particles and forces of nature at energy scales up to 100 GeV. The recent experimental observation of the $W^{\pm}$ and Z^{0} gauge bosons illustrates the successes and delimits the domain of current understanding. There are strong arguments for the occurrence of entirely new phenomena at interaction energies or masses around 1 or 2 TeV. The precise nature of these phenomena is a subject of vigorous debate at present, but the fundamental importance of exploring this mass region is universally recognized.

The investigation of this new energy domain is made possible by a high-luminosity proton-proton collider in the 10-20 TeV range. The technical feasibility of the SSC is the result of a long development of large scale superconducting accelerator technology especially at Fermilab and Brookhaven. In particular, the recent successes of the Fermilab superconducting accelerator have proved that extremely large cryogenic magnet systems can be brought into operation. Much remains to be done beyond this remarkable achievement, but the United States does now have a hard-won, proven technical capability. The SSC, which would build on this technology, is a unique project designed to address physics questions of great significance and promise.

It is clear that a first phase of project planning, cost definition and R&D, followed by a second phase of construction, must be a broad based national effort, centrally managed from the outset. HEPAP concurs with the Subpanel belief that a successful approach will call for an innovative collaboration between the universities, the national laboratories, and industry with the Government. HEPAP recommends that steps to define this process be initiated immediately.

The fourth recommendation of the Subpanel, not to approve the Colliding Beam Accelerator, addresses a very difficult issue in the high energy physics community. It was discussed at length at the HEPAP meeting, and a number of different views were expressed about the importance of CBA, both for its physics promise and as a step toward the SSC. HEPAP also discussed the value of such a facility to provide experimental opportunities before the SSC is completed. An examination of the opportunities at available and projected facilities and a recognition of the need to refocus U.S. resources and manpower led to the conclusion not to proceed with the CBA.

Cognizant of the balance of views on this issue and recognizing the importance of agreeing upon goals that are widely supported by the national high energy physics community, we unanimously endorse the Subpanel's recommendation not to approve the CBA. HEPAP wishes to emphasize that this recommendation is not a judgment against the superconducting magnet program at Brookhaven. On the contrary, the BNL accelerator R&D team and the new BNL management have been highly successful during the past two years. Brookhaven now has expertise and experience that will be an important resource for the national effort that the SSC will require. HEPAP urges the DOE to help Brookhaven retain its vitality in this field as its efforts are redirected.

The third recommendation of the Subpanel, not to approve the Dedicated Collider proposal of Fermilab at this time despite obvious physics opportunities, is accepted as realistic by HEPAP, considering the necessary commitment to the Tevatron I and Tevatron II programs, as well as the R&D effort for SSC.

The implementation of the second recommendation, that current construction projects be completed rapidly, that present facilities and detectors be upgraded, and that all existing facilities be thoroughly utilized, is considered by HEPAP to be essential for the short- and long-term vitality of high energy physics in the U.S. This is particularly important in light of the recommendations not to proceed with the CBA and DC projects.

HEPAP endorses the Subpanel's final recommendation of strong support for advanced accelerator research and development. These efforts are vital to assure the long-range future of high energy physics as an experimental science.

In conclusion, we reiterate our most important recommendation for an immediate initiation of the Superconducting Super Collider project. This project is necessary for a strong and highly creative United States high energy physics program into the next century.

Sincerely,

Jack Sandweiss
Chairman of HEPAP

Attachment
Report of the 1983 HEPAP Subpanel
on New Facilities

STANFORD UNIVERSITY
STANFORD, CALIFORNIA 94305-2196

DEPARTMENT OF PHYSICS

July 8, 1983

Professor Jack Sandweiss
Chairman
High Energy Physics Advisory Panel
Yale University
New Haven, CT

Dear Jack:

I am transmitting to you the final report of the 1983 HEPAP Subpanel on New Facilities. I would like to emphasize the unanimity of its members on all recommendations except the one dealing with the CBA.

The Subpanel wanted to endorse very strongly the rapid construction of the superconducing, multi-TeV, high-luminosity, proton-proton collider. I believe that the Subpanel's recommendation on this issue represents well the desires and aspirations of the whole U.S. high energy community.

As you well know, the CBA issue is a controversial one among American particle physicists and thus it is not surprising that the Subpanel was not unanimous on that point. The majority voted against the CBA approval, but the Subpanel members did agree that the report should reflect opinions on both sides of the issue and this is indeed what is contained in the report.

I sincerely hope that the great deal of effort that the members of the Subpanel have put in during the last few months, and this report, which represents the culmination of that work, will further the goal of a preeminent U.S. high energy physics program during the coming decade.

Sincerely yours,

Stanley G. Wojcicki
Chairman
1983 Subpanel on New Facilties

EXECUTIVE SUMMARY

The 1983 Subpanel on New Facilities of the High Energy Physics Advisory Panel (HEPAP) was formed in February 1983 and completed its report in July 1983. During the intervening 5 months the Subpanel held site meetings at three major accelerator laboratories, heard formal presentations from a number of prominent physicists, received almost 200 letters from members of the high energy physics community, and studied in depth all of the relevant written material. The final recommendations were arrived at after two lengthy deliberative meetings held in June and July at the National Academy of Sciences (NAS) Study Center at Woods Hole, Massachusetts, and at the Nevis Laboratory, Columbia University. The recommendations arrived at by the Subpanel are as follows:

1. The Subpanel unanimously recommends the immediate initiation of a multi-TeV high-luminosity proton-proton collider project with the goal of physics experiments at this facility at the earliest possible date.

2. The Subpanel recommends the rapid completion of current construction projects, Tevatron and the SLAC Linear Collider (SLC), the upgrading of the Cornell Electron Storage Ring (CESR) and associated detectors, and the thorough utilization of all existing facilities.

3. The Subpanel recommends that Fermilab not proceed at this time with the Dedicated Collider (DC).

4. By a majority vote, the Subpanel recommends that the Colliding Beam Accelerator (CBA) project at Brookhaven not be approved.

5. The Subpanel recommends that technology research and development, particularly advanced accelerator research and development, be strongly supported.

The first recommendation not only represents a unanimous view of the Subpanel members, but also appears to reflect the universal aspirations of the U.S. high energy physics community. The Subpanel felt strongly that the construction of a multi-TeV high-luminosity proton-proton collider, referred to here as the Superconducting Super Collider (SSC), provides the promise of important and exciting advances in the field of elementary particle physics. Furthermore, in the Subpanel's opinion, this project lies within the reach of our present-day technology as a result of many years of superconducting magnet R&D in this country. There is good reason to believe that additional R&D work in magnet design and manufacturing techniques will result in substantial cost savings. The Subpanel strongly urges that this project be initiated at the earliest possible date.

The CBA, discussed in the fourth recommendation, represents an issue on which there is no general agreement within the U.S. high energy physics community. Thus, it is not surprising that the Subpanel members could not come to a unanimous opinion on this point. There were areas of agreement about facts and the key issues on which the decision should be based, but in judging these issues and the weights to be given them, Subpanel members came to different conclusions. Most of the concern centered on the CBA potential for important physics discovery and its possible interference with the rapid SSC construction schedule. After lengthy deliberation, the Subpanel voted by a narrow majority against the approval of CBA.

The Subpanel also recommends against the approval of the DC project. Even though the Subpanel was impressed with its physics potential, there was a sentiment against it, mainly on the grounds of interference with the existing and already planned Fermilab program and with the demanding R&D necessary for the SSC.

Finally, the Subpanel felt strongly the need for utilizing the present facilities to their maximum physics potential and for intensified effort in technology R&D, especially in the field of advanced accelerator studies.

SECTION I

PREFACE

The 1983 Subpanel on New Facilities of the High Energy Physics Advisory Panel (HEPAP) was formed in February 1983 to make recommendations relative to the scientific requirements and opportunities for a forefront United States high energy physics program in the next five to ten years. The specific Charge to the Subpanel is attached as Appendix A, and the full membership is listed in Appendix B.

The Subpanel held an initial organizational meeting in Washington, D.C. on February 25 and 26. Subsequent meetings were held at three of the major U.S. accelerator laboratories: Fermi National Accelerator Laboratory (Fermilab) on April 22-24; Brookhaven National Laboratory (BNL) on May 10-11; and the Stanford Linear Accelerator Center (SLAC) on May 20-22. The meeting at BNL was followed immediately on May 12 by a one day deliberative meeting hosted by the State University of New York at Stony Brook. A major meeting of the Subpanel was held on June 5-11 at the National Academy of Sciences (NAS) Study Center at Woods Hole, Massachusetts, during which most of the discussions on the substantive issues took place. The work of the Subpanel was completed at a meeting at Nevis Laboratory, Columbia University on June 29, 30, and July 1.

After the Washington organizational meeting, the chairman of the Subpanel sent an open letter to the full membership of the Division of Particles and Fields (DPF) of the American Physical Society informing them of the existence, role, composition, and schedule of the Subpanel and inviting their input regarding the issues to be debated by the Subpanel. At the same time, letters were sent to the directors of the four accelerator laboratories (BNL, Cornell University, Fermilab, and SLAC) requesting written input about the future plans of their institutions and oral presentations at the subsequent site meetings.

A large fraction of time at each meeting was spent listening to presentations by the laboratory staff and, in the case of the BNL meeting, by a tour of the Colliding Beam Accelerator (CBA) tunnel and magnet factory. In addition, in each instance, a "town meeting" with the high energy physics community was organized by the chairman of the local users' organization. Those meetings gave the Subpanel members an opportunity to hear first hand the opinions and concerns felt by the physicists working at each laboratory and at the same time answer their questions about Subpanel matters. At the Fermilab meeting, the Subpanel also heard presentations by the Cornell laboratory staff and by the director of the High Energy Physics Division at Argonne National Laboratory. At the SLAC meeting, similar presentations were given by representatives of the Lawrence Berkeley Laboratory.

The Subpanel heard presentations and had discussions with the past and present HEPAP chairmen, the present users' organization chairmen of the accelerator laboratories, the present chairman of the Division of Particles and Fields of the American Physical Society, various Federal Government officials, and a number of consultants on various matters of interest.

The Subpanel profited greatly from three different workshops held just before and during the Subpanel's existence. These were:

1. The 1982 DPF Summer Study on Elementary Particle Physics and Future Facilities held at Snowmass, Colorado, on June 28-July 16, 1982;

2. The 1983 DPF Workshop on Collider Detectors held at Lawrence Berkeley Laboratory, February 28-March 4, 1983; and

3. The Technical Workshop on the 20 TeV Hadron Collider held at Cornell University on March 28-April 2, 1983.

In addition, of great value to the Subpanel's deliberations was the Department of Energy (DOE) review of CBA held at BNL on April 11-15, 1983. The two co-chairmen of this CBA review gave a presentation at the BNL meeting of the Subpanel. Various members of the Subpanel attended parts or all of the above workshops and reviews, and written reports of these meetings were made available to all members of the Subpanel.

Additional written material made available to the Subpanel included BNL's proposal for the CBA, Fermilab's preliminary proposal for the Dedicated Collider (DC), documentation presented by the Cornell staff about their plans and activities, and copies of transparencies from most of the presentations given to the Subpanel. In addition, the Subpanel chairman received almost 200 letters from the DPF membership responding to his request for input. These were copied and distributed to all members of the Subpanel. Finally, the Subpanel was given the tables and figures summarizing the demographic data collected during the recent 1982-1983 DOE review of all of its university high energy physics contracts by the Technical Assessment Committee on University Programs (TACUP). Even though the final TACUP report was not completed until after the Woods Hole meeting, the Subpanel did have the opportunity to discuss this review with the TACUP chairman at its June meeting at Woods Hole.

There were several construction projects discussed by the Subpanel. They varied from concrete proposals to very preliminary feasibility studies. In order of decreasing definiteness these were:

1. The CBA proposal presented by BNL which is an evolution of the earlier ISABELLE project for proton-proton colliding rings at an energy of 400 GeV per beam;

2. The DC proposal by Fermilab to construct a proton-antiproton collider with beam energies of 2 TeV;

3. A very high energy (10-20 TeV per beam) hadron-hadron collider--rather than a definite proposal by a given laboratory, this was an idea that originated at the Snowmass Summer Study and was later elaborated at the Cornell Workshop; and

4. A preliminary study of a high energy (about 1 TeV per beam) linear e^+e^- collider presented by SLAC.

Because of the very different status of these various potential projects, quite different criteria obviously had to be used in judging their value to the future of the U.S. high energy physics program.

For the purpose of gathering and evaluating all the relevant information, the Subpanel was divided into three separate task forces, charged with relatively different duties, i.e.:

1. Accelerator Task Force, concerned with technical evaluation of different projects;

2. CBA Physics Task Force, concerned with evaluation of the CBA physics potential; and

3. Overall Program Task Force, concerned with possible scenarios, budgets, and manpower questions.

Even though some discussion of the substantive issues did take place during the laboratory meetings, most of the time before the Woods Hole meeting was taken up with gathering and evaluating different input. The greater part of the last two meetings dealt with formulating recommendations and writing the report.

The remainder of the report is organized in the following manner: Section II contains the Subpanel's recommendations and the comments that elaborate on them. Section III discusses the current status and activity of the field at a relatively non-technical level. An attempt is made to present an overview of the recent discoveries, the current key questions in the field, and an overview of the present and future facilities, both in this country and abroad. In addition, a brief summary is given of the non-accelerator particle physics research. In Section IV, new proposals and ideas presented to this Subpanel are discussed together with technical evaluation. Section V deals with the detailed discussion of the physics topics relevant to the recommendations. In Section VI, a description and evaluation of the existing high energy physics program in this country and of the current U.S. participation abroad is provided. Finally, in Section VII, some aspects of international cooperation in high energy physics are addressed. The Appendices attached to this report give the Charge to this Subpanel and its membership and discuss the U.S. high energy physics manpower and the financial implications of the recommendations.

SECTION II

RECOMMENDATIONS AND COMMENTS

Recommendation No. 1: Superconducting Super Collider (SSC)

The Subpanel unanimously recommends the immediate initiation of a multi-TeV high-luminosity proton-proton collider project with the goal of physics experiments at this facility at the earliest possible date.

Comments:

The Subpanel recommends at the highest priority that a major new project be initiated to design and build a proton-proton colliding beam facility exploiting our superconducting magnet technology with an energy goal of 10 to 20 TeV per beam and completion in the first half of the 1990's. To meet this ambitious time scale, the project must begin in FY 1984 with an intensive research and development phase to establish the precise specifications for the facility.

This project, referred to here as the Superconducting Super Collider (SSC), has captured the minds of high energy physicists everywhere. The conjunction of compelling physics arguments for exploration of the mass region up to a few TeV with the hard won success of large-scale superconducting accelerator technology creates a unique opportunity for the U.S. The SSC is ambitious, but practical. It has immense physics potential, with the very real possibility of truly major advances in our growing understanding of the patterns of particles and the unification of the forces of nature. The SSC can assure a forefront U.S. high energy physics program into the 21st century.

The energy scale is set partly by theoretical expectations. The theory of weak and electromagnetic interactions, which predicted successfully not only the existence, but also the mass of the W and Z intermediate vector bosons, also demands the existence of new phenomena (perhaps a particle, or set of particles) as crucial elements of the theory. While the exact mass scale of this new physics is unknown, general principles impose an upper limit of at most a few TeV. The exploration of this mass range requires proton beam energies of many TeV and high luminosity.

In addition to the physics requirements, the anticipated developments and possibilities elsewhere are also relevant in determining the scope of the project. In the U.S.S.R. a hadron collider, UNK, has been started. Its ultimate energy goal is 3 TeV per beam. The Large Electron-Positron project (LEP) tunnel at the European Organization for Nuclear Research (CERN) can accommodate a proton ring of about 1 TeV per tesla of magnetic field, implying an energy of 4 to 5 TeV per beam if the superconducting magnet production technology developed in the U.S. is adopted.

The Fermilab Tevatron magnet ring is now complete and is being commissioned. This is the first high energy accelerator resulting from more than 20 years of superconducting magnet R&D, which included commercial development in the U.S. of high quality superconducting cable. Unit costs are therefore well known. This technology is now of age. Its full exploitation, leading to an increase in energy of more than a factor of ten over the Tevatron, appears feasible at the present time. Further R&D work in magnet design and manufacturing techniques can confidently be expected to result in substantial cost savings. Although the cost of the SSC cannot be specified precisely at this time, recent studies suggest that construction costs, not including detectors, could be brought down to under $2 billion (FY 1983 dollars).

In brief, we have the following circumstances: good physics reasons for attaining center-of-mass energies beyond 20 TeV, the technical capability for achieving that goal, and healthy competition from abroad. The effort to design and build the SSC, and its ultimate operation, will form the focus of an exciting U.S. high energy physics program over the next decade and beyond.

To achieve these goals, it is imperative that a director be appointed and a management structure and R&D budget be established at the earliest possible date. An innovative approach involving Federal Government, universities, national laboratories, and industry will be necessary.

Recommendation No. 2: Completion of Current Construction Projects and Utilization

The Subpanel recommends the rapid completion of current construction projects, the Tevatron and the SLAC Linear Collider (SLC), the upgrading of the Cornell Electron Storage Ring (CESR) and associated detectors, and the thorough utilization of all existing facilities.

Comments:

An essential element in assuring a forefront program into the early 1990's is the thorough exploitation of existing facilities and of those currently under construction. While recognizing the need to maintain the flexibility to respond to changing physics needs, the Subpanel judges the following elements as essential for optimal utilization of U.S. accelerator facilities:

a. The rapid completion and adequate utilization of SLC and Tevatron facilities should be given the highest priority. The first production of the Z° in e^+e^- collisions is expected at the SLC, allowing precise measurements of its mass and width as well as detailed studies of its decays. Tevatron I has the discovery potential inherent in the first look at $\bar{p}p$ collisions up to 2 TeV in the center of mass, allowing exploration of phenomena well beyond the W and Z mass region. The diverse program at Tevatron II includes extensive studies of charm decays, vertex reconstruction of b decays, precision measurements of nucleon structure functions and of neutral current couplings, determination of CP violation parameters, and searches for new phenomena in beam-dump experiments.

b. The upgrade and utilization of CESR should be vigorously supported. This facility will provide valuable information on b quark spectroscopy, and as a copious source of b decays, it will help to pin down the weak mixing angles and may reveal new CP violating phenomena.

c. The Positron-Electron Project (PEP) and SPEAR facilities at SLAC, and the Alternating Gradient Synchrotron (AGS) at BNL, should be utilized to their full potential and upgraded to the extent warranted by the physics interest. The investigation of the properties of jets at PEP will continue to sharpen the tests of quantum chromodynamics (QCD). Detailed studies of charmed quark spectroscopy and decay at SPEAR will provide important insight into the dynamics of both weak and strong interactions. Rare K decays, carrying information on forces with very small couplings or large mass scales, will be sought at the AGS. Searches for neutrino oscillations will also be carried out there.

In addition, the diverse non-accelerator program should continue to be supported at a level appropriate to evolving physics interests. The significant results on the search for proton decay recently obtained in the U.S. will be refined and extended. The program, in general, addresses fundamental issues not directly accessible at accelerators and provides valuable interaction with other areas of physics such as astronomy and cosmology.

Recommendation No. 3: Dedicated Collider (DC)

The Subpanel recommends that Fermilab not proceed at this time with the Dedicated Collider.

Comments:

The physics potential of the Fermilab DC was found to be very attractive. It would allow approximately twice the utilization, twice the number of interaction regions, and ten times the luminosity of Tevatron I. It would decouple the fixed-target and collider programs at Fermilab. Furthermore, the future option of electron-proton collisions is attractive. Nevertheless, for the following reasons, the Subpanel recommends against the DC:

a. The potential of the DC is insufficient for an incisive attack on the TeV mass region that is deemed essential for the physics and is also within our technical capabilities.

b. An early start on construction of the DC would interfere with the demanding R&D necessary for the SSC; this R&D is sure to involve significant Fermilab participation as a result of their expertise established with the Energy Saver/Tevatron superconducting ring.

c. In the opinion of the Subpanel, construction of the DC at this time would interfere unduly with the proper commissioning and intensive exploitation of the Tevatron programs at Fermilab.

Recommendation No. 4: Colliding Beam Accelerator (CBA)

By a majority vote, the Subpanel recommends that the Colliding Beam Accelerator (CBA) project at Brookhaven not be approved.

Comments:

a. Introduction

The negative recommendation on the CBA project, the result of a series of lengthy and difficult deliberations, represents the view of a narrow majority of the Subpanel. Many arguments on both sides of the issue were heard and analyzed by the Subpanel, and it should be understood that the recommendation was formed on balance, with extensive discussions of the merits on each side. The principal arguments, both for and against approval of the CBA project are described below. There were areas of agreement both about facts and about the choice of key issues on which to base a decision, but in judging these issues and the weights to be given them, Subpanel members came to different conclusions.

b. Opinions in Favor of Approval of CBA

Opinion 1 for Approval of CBA:

There is a significant probability of important discoveries with CBA. There is reason to believe in the existence of new physics at the TeV mass scale. Light bosons related to this mass scale might lie in the 100 to 300 GeV range that would be accessible with the high luminosities available at CBA. In addition, the physics reach at CBA would be much less sensitive to a shortfall in reaching design luminosity than will be the reach at Tevatron I. There may also be important discoveries in systematic, detailed, high statistics studies (e.g., a careful study of K decays at the AGS led to the discovery of CP violation). Such discoveries are more likely on a dedicated machine with a sufficiently large number of interaction regions to allow for diversity of detectors. Moreover, pp collisions may well have important differences from $\bar{p}p$ collisions. Polarized protons at the CBA should also provide interesting opportunities.

At a later stage, when the SSC comes into operation to study pp collisions at a higher energy, the two separate magnet rings at CBA would allow the storage rings to be used for study of heavy ion collisions at a small additional cost. This opens up the possibility of qualitatively new discoveries in a completely unexplored domain that is not accessible with any other type of facility.

Opinion 2 for Approval of CBA:

Brookhaven has solved the magnet problems that had held up this project previously, and the CBA design is now in good shape technically. The civil construction is well under way, and the new management at Brookhaven makes the competent and timely completion of the project likely at a cost consistent with the current estimates.

Opinion 3 for Approval of CBA:

The CBA would significantly increase the breadth of physics opportunities available in the U.S. It would provide six interaction regions full time, compared with two utilized part time at Tevatron I and one at SLC. This would allow for a larger number of diverse detectors which could be crucial in some discoveries. This diversity would also provide a training ground for graduate students and research associates. The existence of CBA could allow a better utilization of Tevatron II at Fermilab by relieving the sole reliance on Tevatron I to do collider physics. The CBA, thus, would be an important element in a strong and vigorous research program in the late 1980's and early 1990's, especially if the time required to complete the construction of the SSC turns out to be somewhat longer than anticipated.

Opinion 4 for Approval of CBA:

The construction, operation, and utilization of CBA would be a natural first step toward realization of the rapid construction and successful utilization of the SSC. Both in accelerator operations and in detector development, the high luminosity pp collisions at CBA would provide valuable experience for the SSC. CBA would train a generation of accelerator and experimental physicists familiar with high luminosity hadron colliders who would otherwise be in short supply in the U.S.

The fiscal interference of the CBA with the SSC should not be significant since CBA construction is expected to be finished by the time SSC construction is ready to begin. With reasonable assumptions about the total budget level for high energy physics over the next few years, both CBA construction and SSC R&D could be accommodated without a significant impact on the rest of the program.

Opinion 5 for Approval of CBA:

Previous long-range subpanels have endorsed the importance of ISABELLE/CBA physics. Some Subpanel members feel that recent physics developments add to the interest in what could be accomplished with CBA, e.g., the study of copiously produced W bosons. In the light of this, termination of the project, after significant expenditures have been made, may have a negative impact on our credibility for obtaining approval of a very expensive new project such as the SSC.

Without the CBA project, vulnerability to any technical problems or delays in Tevatron I construction may be greatly increased.

c. Opinions Against Approval of CBA

Opinion 1 Against Approval of CBA:

The ISABELLE/CBA project was designed as a 400 GeV collider to search for the $W^{\pm}$ and Z° vector bosons and to study particle masses and momentum transfers up to 200 GeV. It was first approved in 1977 and received construction funds in FY 1978. There is no question that it would still be well suited to study this region. However, the technical difficulties with the magnets caused a hiatus of 2 years in the progress of the project. In the meantime, several dramatic new developments have occurred such that the physics opportunities at CBA in 1988 and beyond appear to be considerably diminished. The successful development of the capability to store antiprotons has enabled CERN, building upon the strength of an existing accelerator, to collide protons and antiprotons with an energy comparable to that of CBA. Indeed, during the past few months, both the charged and neutral intermediate bosons have been discovered at CERN. At Fermilab, the Tevatron I collider is expected to be operational one to two years before CBA, with more than twice the energy, albeit with lower luminosity. The existence of the Fermilab facility, together with the CERN collider possibly upgraded in energy and intensity, means that much of the physics accessible to CBA will have been explored before the latter machine could be completed. In addition to the rapid emergence of the $\bar{p}p$ colliders, two high energy e^+e^- colliders, LEP and SLC, are currently under construction. While these facilities are, in general, complementary to, and not competitive with, hadron-hadron colliders, they will address some of the same physics issues. Thus, there are currently four major new facilities, one operating and three under construction, which together address much of the same physics as CBA.

The physics capabilities of several of these devices have been examined in some detail by the Subpanel. The picture that emerges from this study is one of limited opportunity for new and important physics at the CBA. While there is no question that a broad program of useful experimental research could be mounted there, it seems more prudent to require a major new colliding beam facility to have the added discovery potential that comes from entering a new energy domain. The first recommendation of this report is to plan and build such a facility in the United States with sufficient energy to explore physics at a mass scale of a few TeV. Some of the new physics expected at that mass scale may lead as well to lower energy phenomena accessible to CBA. However, this possibility is based on theoretical arguments that are less solid than those that point to the few TeV range. Furthermore, many of these lower energy phenomena will be difficult to observe with any hadron machine.

Opinion 2 Against Approval of CBA:

Even though the serious problem of magnet technology has been resolved, a recent DOE review of the CBA proposal judged the BNL plan for hiring additional top- and middle-management people from the outside to be critically important. The Subpanel agrees with this assessment. Of concern to some Subpanel members is the difficulty of attracting the most competent, well-trained people to a project that is scientifically not sufficiently exciting. Without such people, the project may undergo significant delays and escalating costs.

Opinion 3 Against Approval of CBA:

The diversity offered by the CBA as a high luminosity facility is an important issue to be considered. The additional breadth it would provide must be balanced against the urgent need for a forefront facility (SSC) that will assure U.S. high energy physics a preeminent position, as well as against the importance of the thorough investigation of physics issues at other U.S. facilities. It should be noted that many of the physics questions remaining for CBA could also be addressed at the SSC with comparable luminosity and at more than an order-of-magnitude higher energy. The latter machine might well be operational within six to seven years of the anticipated start of the CBA program and is expected to utilize the capabilities of much of the U.S. high energy physics community, thereby diminishing the appeal of programmatic studies with the CBA.

The diversity of the presently planned program and the need for an enthusiastic and early start on detector development for an SSC facility appear to offer a sufficient range of exciting challenges to maintain the vitality of the field into the 1990's. Talented young people are most likely to be drawn into the field if they perceive it to be at the forefront of discovery. It is felt that the physics potential of CBA would not create that perception.

Opinion 4 Against Approval of CBA:

The general question of available resources, particularly scientific manpower, was also considered. The effective utilization and upgrading of existing facilities, the completion of current construction projects, and the high level of R&D required for rapid progress toward the construction and operation of an SSC collider will place strong and important demands on these resources, whether they be experimentalists, accelerator physicists, or technical staff. The completion of the CBA project and adequate exploitation of its major asset, the high luminosity, would place additional strains on these resources throughout the 1980's and 1990's. It would be very difficult to support the combination of all of these activities at an adequate level and maintain the flexibility to respond to new developments.

Accelerator operations and detector development done in carrying out a high luminosity pp superconducting collider project such as CBA would provide some useful experience for the SSC. However, crucial matters such as beam manipulations in a superconducting magnet environment will be widely explored in the Tevatron project. Injection, beam abort, and slow extraction of a high intensity beam will be integral parts of that project. The Tevatron can also be operated in a high-current proton storage ring mode to simulate closely conditions anticipated in the SSC. Experimentation in the high luminosity CBA environment may be helpful for detector design for the SSC. However, tests of some specific detector components can be carried out at high intensity fixed-target facilities and system performance (triggers, etc.) may, in many cases, be extrapolated from lower luminosity colliders. On balance, the argument that the technology benefits of the CBA are a significant reason for its approval was not found to be persuasive.

Opinion 5 Against Approval of CBA:

The subpanels of 1980 and 1982 recommended continuation of the ISABELLE project, although the report of the 1982 Subpanel endorsed it only at a certain eventual overall funding level. A recommendation at variance with statements of previous subpanels requires comment. There have been surprisingly rapid changes in the field and in the way the important issues are perceived. In the past year and a half, experiments at the Super Proton Synchrotron (SPS) collider have established the existence of the gauge bosons of the standard electroweak model; theoretical understanding has converged on the importance of the TeV mass scale; and the superconducting accelerator R&D of the past ten years has come to fruition. The need for a facility capable of addressing the new higher mass region and the possibility for achieving it changes significantly the importance of the ISABELLE/CBA project. It is felt that CBA now possesses insufficient frontier physics potential to justify its construction in the light of other opportunities. The time has come to turn the advances made by the considerable U.S. investment in superconducting technology to the best possible advantage for physics.

Recommendation No. 5: Long Range Research and Development

The Subpanel recommends that technology research and development, particularly advanced accelerator research and development, be strongly supported.

Comments:

The continued progress of elementary particle physics depends crucially on technological advances. A significant portion of these advances must be driven by the field itself, although commercially driven advances in areas such as electronics, materials and vacuum technology, to name but a few, are also very important. These technological advances are important both for detectors and for accelerators. Vital in both theoretical and experimental physics, high speed computer technology also paces the field. There is a growing sense that, at the frontiers of ultra-fast computing, the needs of basic science and commercial needs overlap strongly, providing new opportunities for direct cooperation and mutual benefit.

Somewhat more than 20 percent of the high energy physics operating funds are normally used for what is called technology R&D (in the DOE budget). By far, the largest part of this pays for R&D in support of approved detector or accelerator construction projects and proposed projects that are quite well defined but not yet approved for construction. This is as it must be. However, only a small fraction of operating resources are devoted to truly advanced technology research. This is the R&D that will provide the new ideas, techniques, and materials that will become the foundation for engineering developments of the future. These, in turn, will permit new advances in physics despite budget limitations. As an example, the basic phenomenon which makes superconducting magnets possible has been known since 1911. The presently accepted Bardeen-Cooper-Schrieffer (BCS) theory was not enunciated until 1957, and its development for engineering purposes is still in progress. The detailed history of attempts to harness superconductivity for use in large arrays of magnets is complex, involving interplay between scientific interests and the metals industry. Only now, more than 20 years after the BCS theory was established, has our advanced R&D yielded an engineering technology that can be widely exploited. Only in recent years has industry, driven by the demand for large accelerator applications, been able to make sufficient quantities of sophisticated conductor with uniform enough quality for extensive applications. As a result, we have now arrived at a point where superconducting magnets are routinely used in large detector magnets and beam transport lines. More significant, the first full-scale high energy accelerator using superconducting magnets has just been brought into operation at Fermilab. One can now predict with confidence that these arduous developments will have their desired profound impact on science.

In the area of accelerator technology, goals are clear but means for achieving them are not. Present superconducting magnet technology will carry circular proton accelerators one or perhaps two generations beyond the Tevatron. Further progress requires magnets with fields of several times ten tesla. Prospects for such an achievement are not bright today. In addition to pushing cryogenic and superconducting magnet technologies to their ultimate capabilities, we need to work as hard as possible on alternate accelerator methods such as laser-driven linacs and plasma beam wave devices. Other new ideas are needed.

For electrons, we are still in need of a technology that can carry us economically into the TeV energy domain. The SLC is a necessary first step for the development of large linear electron colliding beam systems. This project may form the basis of an evolutionary development of standard linear accelerator technology with emphasis on unit cost improvements for the components. Other approaches may require more lengthy R&D but may ultimately prove more cost effective and should, therefore, also be pursued. They include superconducting microwave accelerators, inverse free electron lasers, or the laser and plasma devices alluded to earlier.

Further development of accelerator subsystems such as beam cooling methods, beam focusing methods, and efficient high power microwave and submicrowave generators will also be necessary.

In addition, the Subpanel notes the importance of continued focus on training of new accelerator physicists and continued efforts to draw in new talent and ideas from outside the field.

Mindful of these needs and of our recent history, this Subpanel adds its voice to those of the past HEPAP subpanels in underscoring the urgent need to press forward vigorously in advanced R&D for both detector and accelerator technology.

SECTION III

PARTICLE PHYSICS AND FACILITIES

A. The State of High Energy Physics Today

High energy physics occupies a special position among scientific disciplines. Its objective is to identify the ultimate constituents of matter and to understand the basic forces that bind these constituents to form the world we know. Such questions have been viewed as fundamental since the earliest times. In this century they have led physicists to create very large, very powerful accelerators.

Just as x-rays, with their higher energy or shorter wavelength, permit study of molecules and even single atoms in a way that ordinary light does not, so the fantastically more energetic particle beams from today's big accelerators probe to the very heart of any target put in their path. Part or all of the immense concentration of energy is converted into the creation of matter; a profusion of particles of varying degrees of stability is produced. Large and ingenious detectors receive and record the pattern of particles, often in time intervals measured in billionths of a second. Much of the experimental progress occurs at these large accelerator facilities throughout the world, but important discoveries are also made at frontiers other than the highest possible energies. High energy physics is, thus, somewhat of a misnomer. Particle physics is a better name, with its focus on the fundamental particles and the study of their interactions, whether in the cosmic rays, at big accelerators, beneath nuclear reactors, in deep tunnels and mines, in laser-induced atomic transitions, or in theoretical reconstructions of the origins of the universe.

During the past two decades, our understanding of the fundamental building blocks of nature and their interactions has increased dramatically as a result of remarkable experimental discoveries and concurrent theoretical advances. The picture that emerges has great beauty and simplicity. The constituents of matter are quarks and leptons, point-like, structureless particles of spin 1/2. If gravity is ignored, the forces among these particles are of three types--weak, electromagnetic, and strong. All three arise from the exchange of vector (spin 1) particles, called gauge bosons. Furthermore, the electromagnetic and weak interactions are not separate phenomena. They are described by the unified electroweak theory of Glashow, Weinberg, and Salam. This theory predicted the existence of weak neutral currents discovered at CERN and Fermilab in the early 1970's and, more significantly, very massive charged ($W^{\pm}$) and neutral (Z°) gauge bosons that are in some sense partners of the familiar, massless photon of electromagnetism. The recent discovery of these heavy bosons, with masses on the order of 80 to 100 GeV/c^2, at the CERN proton-antiproton collider is simultaneously a tremendous experimental achievement and a satisfying confirmation of the basic correctness of the now standard unified electroweak model.

The unified electroweak theory of Glashow, Weinberg, and Salam and the theory of strong interactions called QCD form the present-day "standard model" of particles and their interactions. It builds on the immense success of the earlier theory of quantum electrodynamics (QED) which describes the interaction of spin 1/2 particles (electrons and positrons) via exchange of a gauge boson (the photon) and whose initial technical difficulties were mastered in the 1940's and 1950's to produce a theory of tremendous predictive power (agreement between experiment and theory approaching one part in a billion).

In the 1950's and 1960's, the profusion of strongly interacting particles (called hadrons) with particular patterns of production and decay led to the concept of strangeness in addition to the isotopic spin and to arrangement of similar particles in groups (multiplets) according to an internal symmetry which treated isospin and strangeness on an equal footing. The discovery of the Ω^- particle at Brookhaven in 1964 was the climax of this program of classification.

Pioneering experiments on deep inelastic scattering of electrons on protons at the Stanford Linear Accelerator Center in the late 1960's indicated that protons and neutrons contained point-like charged constituents, initially called partons by Feynman. Somewhat earlier it had been suggested by Gell-Mann and Zweig that the multiplets of hadrons could be understood simply if there existed constituents named quarks: two, called u and d, for the attribute of isospin and one, called s, for the attribute of strangeness. There were, thus, different reasons for constituents, one dynamic and one as an explanation of the patterns of known multiplets.

While theorists struggled with the complexity of the strong interactions, more and more experiments, with neutrinos as well as electrons, showed that at short distances (or equivalently for large masses or momentum transfers) the partons inside hadrons behaved as if they were nearly free particles. In the early 1970's, QCD was proposed for strong interactions. It is a gauge theory of the interaction of quarks via the exchange of vector gauge bosons called gluons. It possesses the remarkable property of "asymptotic freedom" in that the overall strength of the coupling between quarks and the gluons becomes weaker as the available energy increases (or the distance of collision becomes smaller). Conversely, it is believed that the force between quarks at large separations is such that two quarks can never be separated. QCD, thus, has the remarkable ability to permit qualitative understanding of the almost free partons of deep inelastic scattering on the one hand and possibly the absence of free quarks on the other. It is a rich and complicated theory from which it is difficult to extract precise predictions, but there is increasing evidence for its basic validity.

In the past ten years, particularly following the dramatic discovery of the J/Ψ particle and its relatives, and, more recently, the upsilon family of states, the reality of quarks as the fundamental constituents of hadronic matter has been established beyond doubt. The massive psi and upsilon particles require the existence of two new quarks, called c and b. The discovery of the tau lepton in 1975 added to the lepton family. The pattern of electroweak interactions has taught us to group the quarks and leptons into families or generations: the u and d quarks, plus the electron and its neutrino, form the first family; the c and s quarks, and the muon and its neutrino, form the next; the b quark and the as-yet-unseen t quark, plus the tau lepton and its suspected neutrino, form a third. Evidence for the first two families is so persuasive that few particle physicists doubt that the t quark and the tau neutrino will be discovered in the next few years, completing the third family.

While the standard model (the unified electroweak theory, plus QCD) provides a wide ranging synthesis, it cannot be the final answer. It does not predict the numbers of families of quarks and leptons, or their masses or mass ratios. Also not determined is the exact relation between the strengths of the weak and electromagnetic interactions, specified by an empirical angle θ_W, called the Weinberg angle. Within the weak sector, the precise form of the weak currents (weak mixing angles and CP violating phases) are left unconstrained by the theory. Finally, there is the all-essential symmetry breaking that causes the gauge bosons that mediate the weak interactions to acquire large masses, but leaves the photon massless. The "*deus ex machina*" is the Higgs mechanism, which has as its consequence the existence of a scalar Higgs particle of unspecified mass. Even the number of these scalars is not determined *a priori*. The only certainty is that their coupling strength to other particles increases with the mass of the particles.

In looking beyond the standard model, there are several major directions. The first is the direction of grand unification. While the strong interactions become weaker at higher energies, the electroweak interactions become stronger. It is thus natural to ask at what energy, if any, do these very different forces become equal. It is remarkable that such an equality is possible, but only at extraordinary energies (of order 10^{14} GeV) because the energy dependence of the interaction strengths varies only logarithmically. Because of this postulated equality, it is possible to imagine the quarks and leptons unified via some grand symmetry, with new and very massive gauge bosons causing transitions between quarks and leptons, as well as the more usual interactions. One beautiful consequence of this new symmetry is the understanding of the exact equality of the magnitudes of the electronic and protonic electric charges. Another is the prediction of proton decay. Other consequences of grand unification are the successful prediction of the Weinberg angle and the possible existence of non-zero masses for neutrinos and of very massive (10^{16} GeV/c^2) magnetic monopoles. Astrophysicists have not been slow to exploit the possibilities of grand unified theories, either for present-day puzzles, such as the "missing matter" in the universe (massive neutrinos?), or consequences of events in the early history of the universe, such as the isotropy of the 3K primordial radiation or the baryon asymmetry. One of the most striking and gratifying developments of recent years is the growing interplay of particle physics and cosmology.

The second major direction being considered is exploration of the so-called Higgs sector. The scalar Higgs particle is a key ingredient in the unified electroweak theory of Glashow, Weinberg, and Salam -- yet no particle is more elusive or ill defined. Many believe that, despite its essential role, it is only an approximation for new dynamical effects to be found at energies a few times the inverse square root of the Fermi weak interaction coupling constant, or roughly 1 TeV. What these new effects might be is the subject of intense speculation at present. Clearly, the search for and study of the Higgs particle (or its equivalent) is of the highest interest and priority.

A third and burgeoning area of theoretical speculation, beyond grand unification, is supersymmetry. In the world as we presently know it, particles of integral spin appear disjoint from those with half-integral spin. But if the quarks and leptons, very different in their interactions though all spin 1/2, can be put on an equal footing via grand unification, why can't we go one step further and unite bosons (integral spin) and fermions (half-integral spin)? This is the aim of supersymmetric theories, in which every particle has a supersymmetric partner, a sparticle, of identical properties except for the opposite kind of spin. Since we have so far not seen any identifiable supersymmetric pairs, the symmetry must be broken. Sparticle partners, if they exist, must have considerably greater masses than the known particles. Bizarre as supersymmetry seems, there are enough attractive theoretical reasons to keep the idea alive and to continue the experimental search for its manifestations.

In the past two decades, experimental and theoretical progress has been astounding. A new level of understanding has been achieved. The sightings of the $W^{\pm}$ and Z° gauge bosons at the CERN proton-antiproton collider are strong proof that we are on the right track. Nonetheless, there are many unanswered questions. Are there more quarks and leptons? Are there other, still heavier gauge bosons? Are their couplings to quarks and leptons only left-handed, or even-handed like the photon's? Where is the Higgs boson? Is it fundamental? Why are neutrinos massless, if they are? How do particles acquire mass? What is the origin of the violation of time-reversal invariance? Are there supersymmetric partners of the known particles? The questions are endless. The list just given has a theoretical tilt. One can ask more practical questions. Does the standard model correctly describe "ordinary" particles at "ordinary" energies? Does QCD correctly describe jet production in e^+e^- collisions, scaling violations in deep inelastic scattering, dimuon production in hadron-hadron collisions? Clearly, much important work remains to be done, both theoretically and experimentally.

It is the nature of our field that investigations at one high energy facility provide answers to some questions yet raise new questions that can only be answered at a different, generally more powerful facility.

From the early synchrocyclotrons to the Cosmotron, then the Bevatron, the AGS at Brookhaven, the Zero Gradient Synchrotron (ZGS), the electron linac at SLAC, Cambridge Electron Accelerator (CEA), the Fermilab 400 GeV accelerator, SPEAR, CESR, and PEP, the proton and electron accelerators and colliders have addressed important questions and provided significant and often startling answers over the years. The progress of the field is paced chiefly by the rate of discovery at these facilities.

The dominant role of U.S. high energy physics in the three and a half decades since the end of World War II has been the result of the enlightened support of the field by successive Administrations and the U.S. Congress. Its flourishing has been a source of pride among physicists, and surely also throughout the nation. It has provided a unique measure of cultural enrichment and intellectual growth for the whole world. That is not to say that others have made no important contributions. On the contrary, Western Europe, in particular, has made impressive strides in the last decade, with the PETRA storage ring at Hamburg and the SPS proton-antiproton collider at CERN, near Geneva, Switzerland. High energy physics is, in fact, a great intellectual and idealistic endeavor that knows no national boundaries. We rejoice at others' discoveries, as they do at ours. United States high energy physics at home and abroad builds on a strong tradition of vigor and excellence that should be maintained and nourished in the coming decades.

B. Survey of Existing and Approved Experimental High Energy Physics Facilities

The experimental program in high energy physics depends in very large measure on the existence of large central facilities serving an international community of physicists whose proposals for experiments are approved on the basis of peer review. The diversity of these facilities is an important factor in the success of the high energy physics program world-wide. In the following paragraphs, a survey of existing and approved facilities is given in order to describe the major experimental thrusts at present. Section III (C) surveys the pending proposals for new facilities throughout the world.

The accelerator facilities themselves are either hadron or lepton machines, with the hadrons being protons and/or antiprotons (or sometimes heavier nuclei) and the leptons being electrons and/or positrons. A further subdivision is into accelerator and storage ring, with the same structure sometimes serving both functions. Present accelerators usually have fixed-target programs in which the primary beam and secondary beams are utilized on numerous target-detector complexes to accomplish a wide variety of specific physics objectives. An accelerator also may serve as an injector into a collider (storage ring) facility with several interaction regions where experiments are mounted.

1. Hadron Accelerators, Fixed-Target

While a number of important experiments in particle physics are done on medium energy hadron machines, the bulk of the program on hadron machines is at facilities whose beam energies are greater than 25 GeV. In order of increasing energy, these are:

a. Alternating Gradient Synchrotron (AGS). Sited at Brookhaven National Laboratory, the AGS accelerates protons to approximately 33 GeV. Since completion in 1960, the AGS has had a distinguished record of important discoveries and presently has the capability of a diverse program of physics with its secondary beams of neutrinos and mesons. Among the physics questions presently addressed are elastic muon-electron elastic scattering, neutrino oscillations, and meson spectroscopy (especially the so-called glueball states).

b. Proton Synchrotron (PS). Similar in energy to the AGS and completed late in 1959, the PS, located at the European international laboratory, CERN, Geneva, Switzerland, is an immensely versatile machine. It accelerates protons, antiprotons, deuterons, and alpha particles more or less on demand, serving as injector to CERN's higher energy accelerators and storage rings, as well as providing beams for its own fixed-target program. It is being modified to serve also as the injector of electrons and positrons for LEP (see below). It provided the neutrino beam to the bubble chamber, Gargamelle, for the discovery of weak neutral currents in 1973.

c. Serpukhov 76 GeV Accelerator. Completed in 1967, the 76 GeV proton accelerator at the Institute for High Energy Physics, Serpukhov, USSR, has a fixed-target program with meson and antiproton beams. Experimental facilities include two large bubble chambers (Mirabelle and SKAT). A new 32 GeV antiproton beam and a neutrino beam are currently under construction.

d. Fermilab 400 GeV Synchrotron. The highest energy proton accelerator in the U.S. is the 400 GeV synchrotron at the Fermi National Accelerator Laboratory, Batavia, Illinois. Completed in 1972, it has served for 10 years as the primary U.S. facility for fixed-target hadron and muon physics at energies up to 400 GeV. Its primary beam intensity is several times 10^{13} protons per pulse. The research program in its three laboratory areas encompasses meson production, scattering, and decay; neutrino interactions; inelastic muon scattering; muon pair production; and high energy photoproduction experiments. Among its important achievements are the discovery of the upsilon particles (bound states of heavy b quarks at about 10 GeV), detailed measurements of neutrino interactions at high energies, photoproduction of charmed baryons, and extensive measurements of elastic and inelastic scattering of hadrons. This accelerator is an integral part of the new Fermilab 0.8 to 1.0 TeV accelerator, the Energy Saver/Tevatron, presently being commissioned (see below).

e. Super Proton Synchroton (SPS). CERN's largest accelerator, at present, is the SPS with a maximum proton energy of 450 GeV for fixed-target physics. Completed in 1976, the SPS is roughly similar to the Fermilab 400 GeV accelerator. Its beam intensity is normally in excess of 10^{13} protons per pulse. It serves two large experimental areas with intense hadron, neutrino, and muon beams whose energies extend up to 200 to 300 GeV. The European community at CERN has executed an impressive physics program with the SPS. Noteworthy achievements include the detailed study of scaling violations in the electromagnetic structure functions through inelastic muon scattering, and production and decay properties of heavy quark mesons. Since 1981, the SPS fixed-target program has had to be cut back to permit operation of the SPS in a proton-antiproton collider mode (see below).

f. Energy Saver/Tevatron. The Fermilab Energy Saver/Tevatron consists of the original main ring of conventional magnets which will operate at 150 GeV to inject into a new ring of superconducting magnets in the same tunnel capable of further acceleration to 1 TeV. This accelerator, unique as the first large superconducting accelerator in the world, is being commissioned in mid-1983, with its first operation for physics scheduled for the fall of 1983. The complex of the Energy Saver/Tevatron, an extraction system, and the network of external beam lines upgraded for 1 TeV operation is known as Tevatron II. It serves the upgraded Meson, Neutrino, and Proton laboratories of the fixed-target program at Fermilab and is scheduled to be completed by 1985, although experiments in some areas will operate in late 1983. After 1986, fixed-target operation will be interlaced with the collider program (see Fermilab Collider below).

2. Hadron Colliders

Hadron-hadron colliders were pioneered at CERN. At the present time, only two such colliders exist, both at the European laboratory. Two more are under construction. These facilities are:

a. Intersecting Storage Rings (ISR). Completed at CERN in 1971, the ISR consists of two rings of protons of 31 GeV maximum energy. There are six experimental intersection regions where the beams cross at an angle of 15 degrees. The circulating beam current is typically 40 A (58 A maximum) with a record luminosity of 1.4 x 10^{32} cm^{-2} sec^{-1}. With center-of-mass energies from 20 to 60 GeV, experiments at the ISR probe an energy domain only accessible with 0.2 to 2 TeV beams on fixed targets. Total cross sections rising with energy were first studied at the ISR, as was the behavior of proton-proton scattering out to large momentum transfers. Particle production was studied in detail, with inclusive measurements establishing clearly the existence of target and projectile fragmentation regions, the central plateau, and the dominance of limited transverse momenta. More recently, large detectors have studied hadronic jet production, jet-jet correlations, and rare processes at large transverse momentum. Some running has been done with protons and antiprotons, protons and deuterons, protons and alpha particles, and two beams of alpha particles to compare with proton-proton interactions. The ISR is scheduled to shut down completely in 1984.

b. CERN Proton-Antiproton Collider (S$\bar{P}$PS). The CERN S$\bar{P}$PS collider consists of an antiproton source and an accumulator ring, the PS as a booster-injector for both protons and antiprotons, and the SPS, where counter-rotating beams of protons and antiprotons are first accelerated to 270 GeV, then stored and allowed to collide with a center-of-mass energy of 0.54 TeV at two interaction regions. Approved for construction in 1978, the S$\bar{P}$PS collider first operated for physics in the fall of 1981. The luminosity has now reached 2 x 10^{29} cm^{-2} sec^{-1}. Its running is alternated with SPS fixed-target operation on a roughly half-yearly cycle. By the end of 1982, the $W^{\pm}$ intermediate vector boson had been observed, while in May 1983 the first evidence of the Z° neutral vector boson was reported. The impressive cleanness of the events with large transverse energy encourages the belief that important physics at TeV colliders may be relatively easy to disentangle despite the very large number of particles produced.

c. Fermilab Collider (Tevatron I). Approved and under construction is the Fermilab proton-antiproton collider. In addition to the Energy Saver/Tevatron, the collider (known as the Tevatron I project) consists of an antiproton source, a stretcher ring for phase-space manipulation of the particles, and an antiproton accumulator ring. The main and superconducting magnet rings will be used for acceleration and storage of the counter-rotating beams of protons and antiprotons, each of energy 0.8 to 1.0 TeV. The beams will be made to collide at a center-of-mass energy of 1.6 to 2.0 TeV in one or two interaction regions. The collider is expected to be commissioned in 1985-86 and in regular physics operation on an approximately half-time basis by 1987. The luminosity is expected to be in excess of 10^{30} cm^{-2} sec^{-1}.

d. IHEP Accelerating-Storage Complex (UNK). The USSR high energy physics program at Serpukhov includes construction, in stages, of a large accelerator storage ring complex, UNK. Its first stage consists of a 21 km circumference ring of conventional magnets capable of accelerating protons to an energy of 0.4 TeV. Present plans include fixed-target operation, with neutrino, muon, and hadron secondary beams from the 0.4 TeV protons. Civil construction has just barely begun, and prototype dipoles are being produced. The tunneling is estimated to take 8 years. The second stage will add a ring of superconducting magnets of 5 tesla maximum field, capable of an energy of 3 TeV. Collisions of the 0.4 TeV and 3 TeV beams will give a center-of-mass energy of 2.2 TeV. The tunnel size is such that another ring of superconducting magnets could be added as a storage ring. Then, a center-of-mass energy of 6 TeV could be achieved. Luminosity of 10^{32} cm^{-2} sec^{-1} is projected. R&D on the superconducting magnets is being pursued jointly with French scientists.

3. Electron Accelerators, Fixed-Target

At present, the only electron linear accelerator in regular use for high energy particle physics research is the Stanford Linear Accelerator at Stanford University. It presently accelerates electrons to energies up to 30 GeV, with planned improvements that will yield 50 GeV by 1986, for injection into the arcs of the SLAC Linear Collider. Its extensive fixed-target program has included use of the primary electron and secondary photon and hadron beams for pioneering experiments in inelastic scattering, for photoproduction, and for meson spectroscopy. At present, the program is less extensive, in part, because the linac functions as a source of electrons and positrons for two electron-positron storage rings, SPEAR and PEP.

4. Electron-Positron Colliders

Almost non-existent before 1970, high energy electron-positron storage ring/colliders now number more than six, with at least four more under construction. The basic components of such a facility are a source of electrons and positrons, an accelerator (linear and/or circular), and one or more storage rings, sometimes serving also as an accelerator. In the final storage ring, the counter-rotating beams of electrons and positrons collide at several interaction regions around the ring.

The processes most widely studied with electron-positron colliders are the annihilation or single-photon reactions in which the total energy of the colliding particles is effective in producing hadronic matter (or heavy leptons) via a virtual photon. Because of the complete conversion of the initial energy, electron-positron collisions are much more effective in exploring a given mass or momentum transfer scale than hadron-hadron collisions of the same energy. A rule of thumb is that an electron-positron collider of one sixth to one tenth the center-of-mass energy of a hadron machine will explore the same general domain of "hard" processes or heavy particle production. The laws of nature and the limits of technology have apparently conspired to permit the concurrent creation of such electron-positron and hadron facilities with roughly equivalent physics potential--tens of GeV e^+e^- machines and hundreds of GeV proton accelerators in the 1970's, 100 GeV e^+e^-, and 1 TeV proton machines in the late 1980's.

The following paragraphs describe briefly the existing or approved e^+e^- facilities in order of increasing maximum energy.

a. SPEAR. SPEAR is located at the Stanford Linear Accelerator Center. It has two interaction regions and a maximum energy per beam of 4.2 GeV, but much of its running has been below 2.5 GeV per beam for charmonium spectroscopy and charmed meson and baryon production. The luminosity at a center-of-mass energy of 3.7 GeV is 10^{31} cm^{-2} sec^{-1}. For at least four years, beginning in late 1974 with its discovery of the psi family of hadronic states, SPEAR and its counterpart, DORIS, at Hamburg were the focal points of much exciting progress in particle physics. The discoveries made real the belief that hadrons are made up of quarks and that there was a potentially correct theory of strong interaction dynamics (QCD). In recent years SPEAR continues a productive program in high energy physics at one interaction region, with beam time shared equally with the Stanford Synchrotron Radiation Laboratory.

b. Beijing Electron-Positron Synchrotron (BEPS). Physicists in the People's Republic of China are designing and building an electron-positron storage ring to cover the energy range of the psions and the lowest mass charmed mesons with substantially higher luminosity than is presently available at SPEAR. This facility, which should be completed in the next few years, is intended to provide China with a competitive machine in the SPEAR energy range.

c. DORIS. The storage ring, DORIS, is located in Hamburg, Germany, at the Deutsches Electronen-Synchrotron Laboratory, DESY. Originally placed in operation in 1974, DORIS has recently been upgraded to 5.6 GeV per beam in order to study the upsilon and B meson region. It has two interaction regions and a luminosity of the order of 3 x 10^{30} cm^{-2} sec^{-1}. In the mid- and late-1970's, experimenters at DORIS competed with those at SPEAR in exploring the spectroscopy of the charmonium region of center-of-mass energies from 3 to 5 GeV. The present program is focussed on upsilon spectroscopy.

d. VEPP-4. The VEPP-4 storage ring, with a nominal maximum energy of 7 GeV per beam, is located at the Institute of Nuclear Physics, Novosibirsk, U.S.S.R. It has three interaction regions and a luminosity of roughly 10^{29} cm^{-2} sec^{-1} at a center-of-mass energy of 10 GeV. Its research program emphasizes the upsilon states and the use of transversely and longitudinally polarized colliding beams.

e. CESR. Completed in 1979, the Cornell Electron Storage Ring (CESR) has a nominal peak energy of 8 GeV per beam, but it regularly operates in the 5-6 GeV regions for upsilon and B region physics. The present luminosity is 10^{31} cm^{-2} sec^{-1}. There are two interaction regions, one equipped with a large 4π magnetic detector and the other with an electromagnetic calorimeter without a magnet. Experiments at CESR have explored the rich spectroscopy of the very narrow upsilon states below the "beauty" flavor threshold, as well as the production and decay of pairs of B mesons above threshold.

f. PEP. The positron-electron collider, PEP, is one of the facilities at the Stanford Linear Accelerator Center. Completed in 1980, its maximum energy per beam is 18 GeV. Most of the operation so far has been at 14.5 GeV with a luminosity of the order of (1 to 3) x 10^{31} cm^{-2} sec^{-1}.

Five of the six interaction regions have large detectors in place. The physics program encompasses one- and two-photon processes, with detailed studies of jets, heavy hadron production, QCD, and measurements of decay properties of heavy leptons and charm (heavier flavor) particles. Sensitive searches can be made for charged particles with masses less than 10 to 15 GeV. Much interesting two-photon physics--production of hadronic states of even charge conjugation, measurement of the photon-photon total cross section, study of the photon form factors--can also be done.

g. PETRA. The accelerator-storage ring, PETRA, was completed in 1978 as part of the facilities at DESY. Its peak energy per beam is presently 20 GeV, with improvements under way to raise it to 22 GeV. The luminosity (at 16 GeV per beam) is roughly (1 to 2) x 10^{31} cm^{-2} sec^{-1}. The broad research program, begun 2 years ahead of PEP, includes the pioneering studies of annihilation events consisting of two and three jets of hadrons to learn about and test QCD, searches for unexpected charged particles, and exploration of the electromagnetic properties of heavier leptons and their decay modes.

h. TRISTAN. Approved in 1981 and under construction at the Japanese National Laboratory for High Energy Physics (KEK), TRISTAN is an electron-positron accelerator-collider with four interaction regions. The beam energies are in the range 25 to 30 GeV and the design luminosity is 10^{31} cm^{-2} sec^{-1}. It is scheduled for completion in 1986 with two large detectors for the initial round of experiments. Its program is expected to stress electroweak interference effects, study of quark and gluon jets, and toponium spectroscopy if the $t\bar{t}$ bound states are within its reach of center-of-mass energy.

i. LEP. Approved by the CERN Council in late 1981, the European 50 GeV x 50 GeV very large (27 km circumference) electron-positron storage ring known as LEP, is now under construction. It is scheduled for operation for physics in 1988. The initial phase will have large experiments mounted in four of the six interaction regions of the machine. The design energy and luminosity of the first phase are aimed at phenomena to be uncovered in the formation and decay of the neutral Z° intermediate vector boson whose mass is between 90 and 100 GeV. One of the triplet of electroweak bosons (W^{+}, W^{-}, Z°), the Z°, is a "heavy photon" mediating the weak neutral interactions of leptons and hadrons in a manner similar to the role of the photon in purely electromagnetic interactions. Its recent discovery at approximately the predicted mass is an experimental tour-de-force at the CERN S$\bar{p}$PS collider and provides a crowning confirmation of the general validity of the standard model of the unification of electromagnetic and weak interactions.

Although the first observations of the Z° (and $W^{\pm}$) have been made at a hadron collider, LEP and other e^+e^- colliders of comparable energy can study in great detail the numerous decay modes of the Z° and bring out phenomena that would be extremely difficult, if not impossible, to extract with the hadronic machines. The whole high energy physics community looks forward to these exciting physics opportunities.

j. SLC. The SLAC Linear Collider is a research and development project exploring the path to very high energy electron-positron linear colliders and having a timely physics payoff as well. The aim is to develop the technology for producing the colliding micron-sized beams of electrons and positrons at energies of hundreds of GeV or even TeV. The SLC project utilizes the Stanford Linear Accelerator and associated damping rings and positron source to inject 50 GeV pulses of electrons and positrons into the left and right arcs that meet at a single interaction point (imagine an unstrung tennis racket with the linac as handle and the interaction point top center). The physics goal is study of Z° physics, just as at LEP. Necessary civil construction is authorized for FY 1984 with planned completion of the SLC by early 1987.

C. Proposed and Contemplated Facilities.

The laboratory frontier of particle physics is continually advancing toward higher and higher energies because such energies are necessary for probing ever more deeply into the "interiors" of nature's fundamental building blocks. When physicists explore the length scales of 10^{-15}m, corresponding to the size of a proton, they find new matter in the form of mesons produced from the energy of collision. At scales ten times smaller in length (ten times higher in energy), they find heavier, very stable mesons, and at higher energies, they find evidence of point-like constituents (quarks) inside protons and other hadrons. At length scales of the order of 10^{-18}m (or energies of order 100 GeV), they find the electroweak bosons. What is expected at still higher energies? Part (A) of the present section mentions some expectations. Section V elaborates the theme. Despite the impressive successes of QCD and the unified electroweak theories, there is uncertainty and debate as to what will be seen at 10 to 100 times higher energies. Energy is the main frontier. As it is pushed higher, predictions stand or fall, but always new and unexpected phenomena occur. Physicists learn new patterns and new relationships. Understanding grows and a more complete picture of the laws of nature emerges.

Because of the quest for deeper and deeper probings, the impressive array of existing facilities must always be upgraded and augmented with new, more powerful machines. Later sections address the capabilities of existing and proposed or contemplated facilities in more detail. The present section offers only a brief summary of the major proposals or ideas of which the Subpanel is aware.

1. Hadron Colliders

In the category of hadron colliders, there are four projects in various states of definiteness. The first is the CBA formally proposed by BNL. The CBA is a high-luminosity proton-proton accelerator storage ring using superconducting magnets up to 5.5 tesla, providing 0.4 TeV protons in each beam and collisions of 0.8 TeV center-of-mass energy with a design luminosity of 2×10^{33} cm^{-2} sec^{-1} in three interaction regions (and one tenth of that in the other three). The CBA utilizes the civil construction of the ISABELLE project. The construction of the machine itself is projected to take three or four years, following one and a half years of further R&D. The CBA project and its potential physics capabilities are discussed in detail in Sections IV and V.

Another hadron collider, described in a preliminary proposal from Fermilab, is the DC. Based on a modest evolution of the existing superconducting magnets used in the Energy Saver/Tevatron, the DC is a proton-antiproton accelerator-collider with center-of-mass energy of roughly 4 TeV (2 TeV/beam) and a luminosity of more than 10^{31} cm^{-2} sec^{-1}. The DC would occupy a separate tunnel of 2 or more km radius and would employ the Tevatron complex as the injector. Its existence would eliminate the need for the Tevatron complex to operate in the collider mode (Tevatron I); fixed-target operation could proceed with interruption only occasionally to fill the DC with antiprotons and protons. Aspects of the physics program at the DC are discussed in Section V.

The third contemplated hadron-hadron facility is an augmentation of CERN's electron-positron project, LEP. The 27 km circumference tunnel for LEP is designed to be large enough in cross section to accommodate one or more additional rings of magnets. The proposal is to install a ring of high-field superconducting magnets for a proton-antiproton collider. With 10 tesla magnets, one could have 8 to 10 TeV beams and so reach 16 to 20 TeV center-of-mass energy. CERN has the antiproton source capability. Research and development is beginning on magnet components. With appropriate funding, the ring and its cryogenics could probably be built and installed over a period of five years since LEP will not operate year round because of power costs.

The most ambitious hadron-hadron collider presently discussed is a 20 TeV x 20 TeV proton-proton machine of high luminosity. The accelerator ring would be very large--approximately 20 km in diameter for 8 tesla magnets or 50 km in diameter for 3 Tesla magnets. The physics potential of such a facility is tremendously exciting, as is discussed in Section V. There are great technological challenges, not in principle, but in engineering and manufacture, in order to lower the cost and achieve the required reliability. These aspects are discussed in some depth in Section IV.

2. Electron-Proton Colliders

There is one electron-proton collider facility proposed. That is the HERA project at DESY, Hamburg, Germany. It presently has conditional approval and should begin construction within two years. The facility will have 30 GeV electrons and positrons and 820 GeV protons from a superconducting accelerator. In collisions, these particles provide in excess of 300 GeV in the center-of-mass and corresponding momentum transfers, permitting exploration of lepton-hadron interactions at extremely high energies. For comparison, a 500 GeV muon beam on a fixed-target attains only 30 GeV center-of-mass energy. It is hoped that the electron ring can be functioning in 1987-88, with electron-proton collisions in 1990-91.

It should be added that the Fermilab DC project incorporates an electron-proton option with 10 or 20 GeV electrons. The layout at LEP would permit electron-proton collisions with the SPS proton beam, although this is not currently planned.

3. Electron-Positron or Electron-Electron Colliders

The LEP project at CERN has been proposed in phases, with the approved Phase I already under construction. Phase II is the gradual addition of superconducting radiofrequency cavities to boost the energy to 125 GeV per beam, well above the energy necessary for $W^{\pm}$ pair production. The time scale for Phase II is set by the pace of the cavity developmental work and of the funding.

In the U.S., the research and development on linear colliders at SLAC has led to discussion of very large linear colliders. The goal is a collision of micron-sized beams of electrons and positrons (or electrons) with energy of a few TeV. Just as for the 20 TeV hadron collider, the scale is very large--50 km linacs and power consumption of hundreds of megawatts are mentioned. Clearly, much research and development on high-gradient accelerating structures, energy storage devices, and other required components must be done to make such an exciting facility a reality.

At Novosibirsk, U.S.S.R., a similar proposal has been aired in a speculative way. The goal is a linear collider with 0.5 TeV per beam but with a length of only a few km. Gradients approximately 10 times what are presently achievable would be necessary for such a machine.

D. Non-Accelerator Elementary Particle Physics

Many important results in the field of elementary particles have come from non-accelerator experiments using, for example, cosmic rays or nuclear reactors as sources. Collisions of protons and heavier nuclei with air nuclei in the atmosphere have been studied up to energies of 10^8 TeV. Some of the most sensitive results on neutrino mass limits come from reactors. Deep underground searches for proton decay give information on physics at energies (10^{12} TeV) totally inaccessible to accelerators.

Experiments of this type started the whole field of elementary particles, and they continue to make significant contributions to our understanding of nature.

SECTION IV

DESCRIPTION AND EVALUATION OF THE PROPOSALS AND NEW IDEAS SUBMITTED TO THE SUBPANEL

Several proposals and possibilities for future accelerator facilities were brought before the Subpanel--a proton-proton Colliding Beam Accelerator (CBA) by BNL, a proton-antiproton Dedicated Collider (DC) by Fermilab, an e^+e^- Large Linear Collider (LLC) by SLAC, and a multi-TeV proton-proton collider facility (SSC) which has been widely discussed in various community workshops. Brief descriptions and evaluations of these items follow.

A. Colliding Beam Accelerator

1. Introduction

The CBA proposal for a proton-proton intersecting ring collider at BNL is the successor of ISABELLE with the same basic design parameters, viz,

- Proton Energy:	400 + 400 GeV
- Intersection regions:	6
- Peak Luminosity:	2×10^{32} cm^{-2} sec^{-1} in 3 intersections 2×10^{33} cm^{-2} sec^{-1} in 3 intersections (mini-beta insertions)
- Future options:	electron-proton, heavy ion collisions, polarized protons

The planned completion date, assuming construction were recommended in mid-1983, is October 1987.

ISABELLE construction was authorized in FY 1978. The 1980 HEPAP Subpanel, reviewing the progress of the project, reported that unexpected difficulties had been encountered with the dipole magnets and that an R&D program had been instituted at BNL to correct design difficulties. The 1982 Subpanel of HEPAP reported that, as a result of the R&D program, a new design of dipole magnet had been adopted using Fermilab-style Rutherford Laboratory cable and a bolted-up split yoke design which resulted in 5-foot models of excellent maximum field capability. The 1982 Subpanel expected that the fifteen-foot dipoles would exhibit the same stability and maximum field characteristics as the 5-foot models and noted with approval the continuing R&D program aimed at building one sextant of the magnet system (54 dipoles and 23 quadrupoles) and installing it in the tunnel, ready for tests, by April 1984. By the end of 1984, these magnets would be cooled down and pulse operated to provide an integrated systems test. The present Subpanel, reviewing the progress made during the last year, found that the completion dates for the different items of the magnet R&D program have been successfully maintained and that the "unexpected difficulties" previously reported by the 1980 Subpanel have been overcome.

The present state of the magnet and other principal technical components of the CBA are evaluated below based on these sources: Colliding Beam Accelerator, May 1983 and a presentation of the present state of the CBA project by P. Reardon; Report of the DOE Review Committee on the BNL Colliding Beam Accelerator, April 25, 1983 and a presentation of the latter by its co-chairmen, H. A. Grunder and L. E. Temple, Jr.; a visit and inspection of the work on site; and several discussions with the CBA project staff.

All these reports, presentations, and discussions took place during the session of the Subpanel held at BNL on May 10-11, 1983.

2. Conventional Construction

Seventy-five percent of the civil construction is now complete or under contract to be completed this year. The first phase of construction was begun in April 1979 and was finished by August 1981.

Of the six experimental halls, four are complete and two await authorization. The Service Building, consisting of the cryogenics wing, the compressor structure, and the four-story main building, including control rooms, offices, shops, laboratories, and the rf power supply wing, could be completed this year. The rest of the site work and utilities could also be completed this year.

3. Lattice Design

The CBA lattice design closely follows the successful ISR design with the difference that the insertions are relatively much longer and are matched to the arcs. This design permits a flexible tailoring of the interaction regions to varied physics needs. The beam-beam tune shift is the dominant beam-dynamical factor limiting performance.

Mini-beta insertions have been proposed for three of the six interaction regions to provide luminosities up to 2×10^{33} cm^{-2} sec^{-1} and designs of these insertions, which do not change the beam-beam tune shift, exist. The extra cost of the insertions would be about $5 million.

4. Magnet System

Not surprisingly, given its history, the magnet system currently developed and proposed for the CBA, especially the dipole magnets, has been subjected to close scrutiny both by the DOE Review Committee and, subsequently, by the Subpanel.

The DOE Review Committee came to the conclusion that, in its opinion, "...the CBA magnet, as designed and fabricated, can and does perform as required in terms of peak field, ramp rate, and field quality. The combination of Fermilab-type cable and the CBA bolted yoke design provides adequate operating margin." Having interrogated the Review Committee and the CBA project management on this crucial issue in some detail, the Subpanel sees no reason to differ from this conclusion.

However, the dipoles made so far, all of which are highly satisfactory from the points of view of maximum field and field quality and are constructed according to methods which will be used in production, are not yet being manufactured by the final magnet factory. Furthermore, the trim coils are not yet of the final design, although the most recent design seems satisfactory. There is still much to do before it can be claimed that BNL is producing satisfactory CBA dipoles from its factory.

In addition to the purely magnetic performance of the magnet system, there remain some outstanding questions on its quench protection. The CBA quench-protection system relies on natural propagation of the normal region to distribute the stored energy in a single dipole (1.1 MJ) over sufficient volume to limit local heating to a safe level. In addition, there is a cold diode across each half of the magnet coil to bypass the current of the other non-quenched magnets and allow their energy to be safely drained away into a dump resistor. This "passive" system, if successful, appears simpler, cheaper, and potentially more reliable than the "active" heater system used for the Tevatron magnet. Although no magnet has been damaged in the full-cell tests to date, these tests are, so far, small in number, and many were not made in worst-case conditions. A more detailed critique of the CBA quench-protection system is given in the DOE Review Committee report.

Many components of the final refrigerator are already on hand, and the final acceptance tests could take place in the summer of FY 1984 in time for the sextant test described above.

5. Beam Scraping and Dumping

Due to the concentration of BNL on the magnet system of the CBA and the allocation of R&D funds almost exclusively for that purpose, the development of the other machine components of the CBA has had to mark time during the last year or so. One important problem, which has not had sufficient attention yet, is that of beam scraping and dumping. The particles scraped off the beam, or lost in any way, must be safely absorbed in dumps so that they do not end up somewhere in the magnet system since, about 4 mA of beam locally deposited is sufficient to quench a magnet. The only experience to date in running high-current beams in operating machines with superconducting magnets is that of the ISR at CERN which has eight superconducting quadrupoles in one intersection. Those quadrupoles have occasionally quenched due to being hit by small fractions of the circulating beam. The next experience will come with Tevatron but at much lower beam currents. All these problems have been studied on paper by BNL, but, given the large value of design beam current and the relative fragility of the magnet surrounding the beams, this problem remains the most critical from the point of view of operating such a high-current storage ring machine.

6. Other Components of the CBA (Vacuum, rf, Control & Power Supply Systems)

Progress in the design and development of other major components of the CBA has been held at a low level during the last year or so for the reasons already mentioned. Nevertheless, some progress has been made and the Subpanel did not identify any major technical problems which would be likely to delay the project.

7. Time Schedule and Cost Estimates

Production of the magnet system is a primary critical path element. According to the accelerated construction schedule presented by BNL in June 1983, dipole and quadrupole production could be completed by February 1987. Following that, the whole magnet system would be installed in the tunnel by March 1987 and the first colliding beams could be obtained in October 1987.

An accelerated schedule of this kind presupposes that the first set of experiments could also be conceived, designed, approved, constructed, and installed in time to match the machine schedule. In considering the value of an accelerated schedule for CBA construction, the problem of harmonizing machine construction with preparation of experiments would need careful consideration.

Another limitation on the CBA construction schedule is the time it would take to assemble the construction team, especially the leaders of the groups responsible for the major components. The project leader aims to recruit about half of these leaders from outside BNL but could not do so without a decision to go ahead with the CBA. The Subpanel, in agreement with the DOE Review Committee, is firmly of the opinion that the success of this project and its time schedules would critically depend on building up a team of dynamic and competent leaders and integrating them together as a team under the project leader as fast as possible. This would be just as important as solving the remaining technical problems of the CBA and would represent a major challenge for BNL management.

The cost to complete the CBA machine is now estimated to be (FY 1985 to completion, FY 1983 dollars):

\$218M	Construction (including mini-beta insertions)
53M	R&D and Operation
\$271M	

To this is added \$85 million for the BNL funding of the first set of detectors; it being assumed that an equal amount would be provided by the experimental teams. The total for machine completion, including the BNL contribution to detectors, is, therefore, \$356 million.

The DOE Review Panel, which examined the CBA cost estimates of BNL in detail, was of the opinion that the quoted figure of \$356 million (FY 1985 to completion, in FY 1983 dollars) was adequate to finish the project assuming "effective and aggressive management" and examples were given as to how this might be achieved. The Subpanel also found the cost estimate adequate but assumed that, if the project team mentioned above could be assembled quickly, BNL would endeavor to reduce cost wherever possible during the course of the project.

8. Concluding Remarks

It is not now possible to point to any major technical reason why BNL could not complete the CBA within the proposed time schedule and cost estimate. Certainly, technical problems still exist, but solutions are at hand. The success of the CBA as a project would depend critically on assembling together a dynamic and competent team under the present project leader.

B. Dedicated Collider (DC)

1. Introduction

In a proposal dated May 1983, Fermilab describes the possible construction of a single-ring, proton-antiproton, colliding-beam facility with beam energies up to 2 TeV. Called the DC, the design luminosity is above 10^{31} cm^{-2} sec^{-1}. It could run almost continuously in collider mode freeing the Tevatron to run almost entirely for fixed-target physics. It would be housed in a tunnel 13 km in circumference. The ring would use the Tevatron (1 TeV) as an injector to provide both proton and antiproton beams. In its simplest version, the magnet ring would have a rather high betatron tune (44) in order to meet the luminosity requirement which demands a large number of stored bunches. The strategy proposed to accommodate a large number of bunches is to keep the proton and antiproton orbits separated at all places except in the six interaction regions by means of an electrostatic separation system. The bunched-beam luminosity goal is "greater than 10^{31} cm^{-2} sec^{-1}" which, according to the design is limited not by the expected beam-beam tune-shift, but by the condition that the event rate per bunch crossing not exceed an expectation value of unity. The tune-shift limit is about 4×10^{31} cm^{-2} sec^{-1}. The time during which two counter-rotating bunches intersect is 6 nanosec; the interval between such crossings is 1 microsec.

The designers of DC argue that further design studies may show how to increase the beam energy by up to 25 percent through a better filling factor for bending magnets and slightly higher magnetic fields.

The DC proposal lists two electron-proton options:

a. The addition of a 10 GeV electron storage ring could provide a center-of-mass energy of close to 300 GeV for electron-proton collisions at a luminosity of 6 x 10^{31} cm^{-2} sec^{-1} , comparable with HERA in both of these parameters. Two of the interaction regions would have electron-proton collisions and four would have $\overline{p}p$ collisions.

b. Alternatively, the addition of an electron ring of 40 GeV, housed in the DC tunnel, could provide a forefront facility for electon-proton collisions reaching well beyond HERA's capabilities.

2. Accelerator Technology for the DC

The DC design is based largely on Energy Saver/Tevatron magnet technology. This technology is just now being put to the test as the Energy Saver/Tevatron comes into operation. Thus, we shall know shortly whether this magnet design provides a sound foundation for storage ring construction. The present DC design calls for dipoles longer than those of the Energy Saver/Tevatron by 30 percent and quadrupoles longer by 100 percent. These changes are large enough to require some further magnet engineering development.

Operation of the Energy Saver/Tevatron and Tevatron I, with its antiproton source (the system that would act as injector for the DC), will allow several crucial features of the DC design to be tested well in advance. Furthermore, important data on superconducting magnet longevity and experience on possible system problems will be obtained. In addition, this operation now starting will allow experience to be developed on proton storage rings and on necessary beam manipulations in a superconducting magnet environment. Beginning in 1985-86, the first U.S. experience with $\overline{p}p$ colliding beams will also be acquired which would be valuable in guiding a final DC design.

A critical feature of the DC that cannot be fully tested in Tevatron I is the multi-bunch collision avoidance. Possible coherent instabilities caused by close passage of the bunches could conceivably limit currents and, thus, luminosity below the goals. It should be noted, however, that this possibility is amenable to theoretical analysis; that it will be tested soon at Cornell in an e^+e^- storage ring; and that individual-bunch feedback control appears technically feasible.

While the Tevatron I is a pioneering step in accelerator physics and technology which will be crucial in designing very high energy hadron colliders of the storage ring genre, the Subpanel considers the DC primarily as a further exploitation of that technology for particle physics rather than as a central test of new accelerator technology.

3. Cost and Schedule

The DC proposal projects a total construction cost of $380 million. A preliminary DOE examination of the detailed figures presented by Fermilab agrees generally with the basic costs. The DOE examiners contend, however, that further allowances for engineering, design, inspection and administration, contingency, and pre-operating costs should be included when considering the likely total cost of the DC; they suggest an added $90 million. On the other hand, DC proponents have suggested that, by careful exploitation of existing facilities and equipment, the originally projected costs can be lowered by some tens of millions. Thus, for the purposes of its deliberations, the Subpanel assumed that the DC construction cost would lie somewhere in the $380 million to $450 million range.

The schedule projected by Fermilab for the DC envisions a calendar span of six years for completion, four years of construction preceded by two years of preparatory R&D. Viewed in isolation, this schedule appears technically possible in principle. It is true that the DC layout in a new tunnel minimizes direct physical interference between construction of the DC and operation of the existing complex. Over the next several years, however, Fermilab is committed to an extremely ambitious program of construction, commissioning followed by utilization of several difficult accelerator projects. Given these facts, the Subpanel believes that there is significant probability of interference between DC and the already committed Fermilab program.

C. Superconducting Super Collider (SSC)

1. Introduction

The multi-TeV hadron collider (SSC) under consideration by the Subpanel is an accelerator/storage ring complex to accelerate protons up to energies of 10 to 20 TeV and to collide them producing luminosities up to 10^{33} cm^{-1} sec^{-1}. The principal reference used as the definition of the scope and for the estimation of the costs and the construction schedule for such a facility was the *Report of the 20 TeV Hadron Collider Technical Workshop*, a report *of the study carried out at Cornell University, March 28* through April 2, 1983. That report contains references to earlier technical studies, especially the 1978 and 1979 studies sponsored by the International Committee on Future Accelerators (ICFA).

Many design excursions were examined in the workshop, including single-ring designs, which provide only $\bar{p}p$ collisions, and two-ring designs which provide, in principle at least, pp, $\bar{p}p$, and $\overline{pp}$ collisions, assuming that an antiproton source system (cooler, etc.) is included in the complex. The single-ring $\bar{p}p$ designs are limited by present $\bar{p}$-source technology to luminosities of only 10^{31} cm^{-2} sec^{-1}. If a magnetic field of 5 tesla were chosen, a beam energy of 20 TeV would correspond to a machine radius of about 15 km. It is assumed that the machine will be built on a new site, the real estate for which will be furnished by the host. External beams and the facilities to use them are not included.

The Subpanel believes that the project definition adopted in the Cornell study, while far from precise, is sufficient to establish the approximate time scale upon which we can hope to design and build a multi-TeV machine and is sufficient to estimate its approximate cost.

2. Accelerator Technology for a Multi-TeV Hadron Collider

The fact that a hadron collider of up to 20 TeV per beam is now being enthusiastically considered by the particle physics community is due to the recent successes of superconducting magnet technology at Fermilab and BNL, coupled with the enormous scientific opportunities which would be opened up by these ultra-high center-of-mass collision energies. All approaches to building such a collider require superconducting magnets. Three different classes of magnet designs can be considered: super-ferric magnets (2 to 3 tesla), magnets based on extrapolation of present NbTi conductor technology used at Fermilab and BNL (4 to 6 telsa), and magnets using high-field, Nb_3Sn technology (8 to 10 tesla). A research and development program will be required to choose the appropriate technology and to begin pilot production of magnets. In addition to the selection of a superconducting material, various cryogenic problems will have to be solved including the choice between 2-degree and 4-degree operation and the attainment of very low heat losses from the cryostats.

Accelerator design problems outside the area of magnet technology do not appear to pose any threat to the feasibility of a 20 TeV collider, although all of the subsystems need to be developed actively in parallel with the magnet system. A noteworthy example is the system for beam scraping and dumping. While carried out successfully in the ISR, these beam manipulations are difficult and may prove formidable in this large ring with its superconducting magnet environment. Early experience at the Energy Saver/Tevatron at Fermilab, with its slow extraction system, will be important in first evaluations of this potential difficulty. The Subpanel believes it reasonable to assume that these machine design problems can be solved during the time required to develop the magnets and their production methods. A thorough assessment of these matters must be an early objective of the R&D program.

3. Cost and Schedule

In its deliberations regarding cost and schedule matters, the Subpanel studied the reference materials and heard presentations and arguments from a number of consultants who are experts in superconducting magnets and in construction of high energy accelerator facilities.

The Cornell study gave the estimated cost of a two-ring proton-proton collider to be about $1.7 billion (FY 1983 dollars), provided that the design took advantage of a three-to-four year research and development period to reduce present-day costs. Adding an antiproton capability was said to increase this figure by another $100 million. The unit costs for the magnets embodied in these estimates are much lower than those presently being experienced at Fermilab and BNL. To attain them certain economies and efficiencies in manufacture must be realized through the research and development program and these economies and efficiencies may be difficult--some even impossible--to reach. As the design of the collider is yet in a primitive state, the Subpanel recognizes that the cost of the highest energy facility envisaged in the workshop studies could prove to be substantially higher than those quoted here. Therefore, the narrowing of this cost uncertainty must be a high priority objective of the R&D study.

It is estimated that the cost of the full R&D program, leading to construction of a multi-TeV collider facility, may average $50 million per year (FY 1983 dollars) over the first four years.

With regard to completion time of R&D plus facility construction, the Subpanel heard individual estimates ranging from nine years to fifteen years. After much debate, the Subpanel concluded that the total time to complete must be regarded as uncertain within that range, all factors considered. Two important technical factors are the ultimate scope of the project and choice of magnet technology. If the design relies heavily on proven methods and commercially available technology, the shorter time estimates may be reasonable. If new technology is selected, a somewhat longer time will be required. Selection of scope and technology must rest on early results of the R&D program which cannot be predicted in advance.

D. Large Linear Collider

1. Introduction

A scheme to extend current linear accelerator technology to the 1 TeV range was put before the Subpanel at SLAC. While no definite proposal was made by SLAC, rough examples, based on using standard accelerator waveguides driven by 10 cm or 5 cm microwave power sources, were presented. In this scheme, two opposing, coaxial linear accelerators would accelerate a series of charged particle bunches toward each other in such a way that collisions between the bunches take place in the space between the linacs where sophisticated beam optical systems focus the beams to sub-micron sized spots at multiple collision points displaced laterally from one another. Different bunches collide at different collision points.

2. Discussion

Technical success with such a colliding beam system depends upon the ability to produce and maintain high phase space density bunches in the face of strong space-charge and wakefield forces and on the ability to bring these bunches to a sharp, steady focus of typical dimension 0.1 micron--10 times smaller than that sought in the SLC. In addition, luminosity multiplication, through the self-pinching of the opposing beam bunches during collision, is crucial for the achievement of the postulated luminosity. Some confidence about our understanding of these factors will be gained in the course of bringing on the SLC. It should be emphasized, however, that the beam size and transverse phase-space area of the beams needed for the TeV case are about an order of magnitude smaller than the SLC specifications.

Economic success with this direct evolutionary approach depends on achieving considerable improvement in unit costs, both capital and operating. Major items in the capital cost are the rf sources, including klystrons and modulators; and the accelerator sections, including accelerating and feed waveguides, vacuum systems and supports. The cost estimates presume the successful development of an economical klystron-modulator combination capable of 330 MW peak rf power and 26 kW average power. The currently operating SLAC combination is 35 MW/50 kW. In addition to this technical accomplishment, the estimators assumed that economies of scale would permit klystrons/modulators and accelerator sections to be built at about one half, or less, of current small-quantity prices.

Net conversion efficiency of mains power to beam power for single bunch operation of a conventional SLAC-style linac is considerably less than 1 percent. As average beam power of 10 MW or more will be required to produce useful luminosities at a Large Linear Collider, efficiencies of acceleration must be improved considerably to avoid unmanageable electric power usage. In the examples given, this is supposed to be accomplished by passing several bunches through the linac, rather than just one as in SLC, for each filling of the linac with microwave energy. Means of avoiding long-range wake effects and achieving precision control and division of the bunch trains among several collision areas were designed only schematically and will need further study.

Assuming the necessary technological developments, it was estimated that one might provide, at 2 TeV center-of-mass, a luminosity of 1.5×10^{32} cm^{-2} sec^{-1} at each of six parallel interaction regions with an energy spread of 10 percent, for a cost of about $3 billion with a total laboratory operating power of 500 MW. If the necessary hardware developments could be accomplished, it was further estimated that the use of the shorter 5 cm wavelength might result in an overall facility cost of $2.4 billion, with a corresponding total laboratory operating power of 400 MW.

In addition to the use of such linacs for electron physics, one can imagine their use for electron-proton collisions if a suitable cooled proton source (e.g., cooling ring) could be developed.

The proponents argued that, with sufficient emphasis, the necessary preproposal R&D might be carried out in five years.

SECTION V

PHYSICS DISCUSSION

A. Introduction

The field of high energy physics has changed dramatically over the past fifteen years. A series of remarkable experimental discoveries has combined with the emergence of gauge field theories to provide a broad understanding of elementary particle interactions. The standard electroweak theory has been thoroughly tested at low energies and, with the discovery of the $W^{\pm}$ and Z° vector bosons, it becomes ever more firmly established. QCD continues to provide the best theoretical description of the strong interactions. Even though quantitative tests have proved to be elusive, the existence of phenomena such as weak scaling violation and three-jet events are all nicely explained by QCD.

It is, of course, most important to continue to test QCD and the standard electroweak theory. At short distances, perturbative QCD must be confronted by more detailed studies of scaling violation and jet formation. Low energy properties, such as hadron masses and magnetic moments, are still to be computed using strong coupling methods. The lattice approach, together with the growing use of large computers, offers a good deal of promise and is being actively pursued. The electroweak theory must continue to be probed at low energies and through further studies of the $W^{\pm}$ and Z° vector bosons. It will be especially interesting to bring these measurements to a level of sensitivity that will clearly test the higher order corrections. The vector bosons will be studied at the $\bar{p}p$ colliders at CERN and then at Fermilab, and some detailed high energy tests should emerge from the exploration of the Z° resonance at SLC and LEP. Such studies could also be carried out at the CBA.

By the early 1990's, this vigorous experimental program will have subjected QCD and the standard electroweak theory to ever more stringent testing. Even though surprises may well be encountered, it is not unlikely that our theoretical efforts to understand the deeper questions about these theories will remain without any new direct experimental guidance. The most immediate of these questions is the origin of gauge symmetry breakdown in the electroweak theory. Beyond that, but closely tied to it, is the origin of the fermion masses and mixing angles with which we parametrize both QCD and the electroweak theory. The 20-year old puzzle of CP violation is part of this general complex.

B. Physics at Very High Energies

The mystery of symmetry breakdown in the electroweak theory, that is, the nature of the Higgs sector of the theory, presents an especially important and exciting challenge to experimental high energy physics. The reason is that there are rather general theoretical reasons why the mass scale of this sector of the theory can be no more than a few TeV. While this probably lies beyond the reach of the current generation of colliders, it is certainly within striking distance of a hadron machine of multi-TeV capability.

The experimental excitement is heightened by the fact that we know almost nothing about what will be found there. The Higgs sector has traditionally been described by a set of scalar fields with a renormalizable Lagrangian arranged to produce the desired symmetry breakdown. However, there are reasons to regard this device with some skepticism and to anticipate that the actual underlying dynamics will be rather different. Whatever it is, there is little doubt that further theoretical progress depends critically on finding out. Until we know, ideas such as a grand unification will continue to rest on rather shaky ground.

That the mass scale for new phenomena related to the Higgs sector can be no more than a few TeV can be seen in a variety of ways. If, for example, the conventional scalar field description is employed, the Higgs mass is given by $(M_H)^2 = (\lambda/4\pi^2)\ (2\sqrt{2}\pi)^2\ /G_F$, where $\lambda/4\pi^2$ is roughly the perturbation expansion parameter for the renormalizable ϕ^4 interaction. Since $G_F^{-1/2} = 300$ GeV, the strong coupling limit $\lambda/4\pi^2 \rightarrow 1$ corresponds to M_H approaching a few TeV. More generally, the bound imposed by unitarity on certain partial-wave amplitudes implies that new phenomena must occur at a scale no larger than this.

Although a value of M_H considerably less than 1 TeV is possible, and such a light Higgs boson should certainly be searched for, there is no obvious mechanism for stability of the small mass against renormalization corrections. Specific dynamical models which address this problem do indeed conclude that a few TeV is the natural mass scale of the Higgs sector. Although none of these models is completely satisfactory, they have the common feature of also predicting the existence of some additional particles with masses considerably smaller than the primary scale. These are particles that receive their masses only through radiative corrections. It is possible that they may be accessible at facilities probing the mass range of a few hundred GeV.

The origin of electroweak symmetry breaking is only one of many puzzles that define the cutting edge of our field. However, because of its importance and accessibility, it imposes a clear minimal requirement on our long-range planning. The next high energy facility to be designed and constructed in the United States should be comfortably able to make a few TeV available for new particle production.

Either an e^+e^- facility with a center-of-mass energy of several TeV or a hadron collider with a center-of-mass energy in the tens of TeV range would allow an exploration of the TeV region for hard collisions. The physics capabilities of these two options are equally attractive and somewhat complementary. The results of the CERN $\overline{p}p$ collider indicate that hard collisions at very high energies are easier to isolate than had been anticipated. Hadrons provide a wider variety of parton initial states, which allows for a greater diversity of phenomena but loses the measurement advantage of the well defined initial state in e^+e^-. Because the current state of technology favors the hadron collider (see Section IV), only this device will be considered in the remainder of this section.

A multi-TeV collider (SSC) will no doubt reveal much more than the mechanism for electroweak symmetry breaking. Our current ideas center on phenomena such as heavy fermions and gauge bosons, possible quark and lepton substructure, or perhaps evidence that supersymmetry plays some role in the real world. The experiments will no doubt prove this list to be one of limited imagination. Surprises have always been encountered when we enter a completely new energy regime, and there is no reason to expect them not to happen again.

At the very least, however, a list of possibilities can be used to calibrate the power, or reach, of a multi-TeV hadron collider. Two typical reactions of interest are the production of heavy quark pairs and the production of a heavy, neutral gauge boson, Z'. Table 1 below gives the maximum mass that can be reached in each case if 100 events are required. Two different energies and two different integrated luminosities are considered. The smaller energy, $\sqrt{s} = 10$ TeV, is perhaps the minimum required to have a good chance for access to a TeV Higgs sector, while the larger energy, $\sqrt{s} = 40$ TeV, would allow a more thorough search and appears to be a reasonable target value for a first-generation multi-TeV hadron collider.

Whatever the physics of the TeV energy region turns out to be, its exploration will inject sorely needed guidance into attempts at a deeper theoretical description of nature that, at present, are of necessity highly speculative.

Table 1

Maximum Quark Mass Reached if 100 Quark Pairs Must be Produced*

$\sqrt{s}$, TeV	$\int L dt$, cm^{-2}	Quark Mass, GeV
10	10^{38}	750
10	10^{40}	1250
40	10^{38}	1500
40	10^{40}	3400

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*From information presented to the Subpanel by I. Hinchliffe.

Table 1 (continued)

Maximum Z' Mass Reached if 100 Z' Bosons Must be Produced*

$\sqrt{s}$, TeV	$\int Ldt$, cm^{-2}	Z' Mass, GeV
10	10^{38}	1400
10	10^{40}	2400
40	10^{38}	2900
40	10^{40}	7000

C. Physics at the 50 to 200 GeV Mass Scale

The first assault on this energy range at the CERN SPS collider has clearly confirmed the existence of the $W^{\pm}$ and Z^{0} bosons. With the completion of Tevatron I, SLC, and, somewhat later, LEP, there will be four accelerators with access to these energies. The CBA would operate within this range but at much higher luminosity and with many interaction regions, while the proposed DC would penetrate to somewhat higher energies. A sense of the physics opportunities at these energies, as well as the relative strengths of the devices, can best be given by examining some of the phenomena that might be explored.

The Subpanel devoted considerable attention to this task. Production rates were estimated for a large number of interesting processes, and this work was supplemented by a variety of resource materials made available to the Subpanel. Among these were the proceedings of the 1982 DPF Snowmass Summer Study, the Fermilab DC Proposal, and the Brookhaven CBA Proposal together with supporting documents. Our studies are summarized below by some paragraphs on several representative processes that might characterize physics at these energies. The list is somewhat arbitrary, proceeding from tests of the standard electroweak theory to jet studies and QCD, and then to some more speculative topics such as new particle searches and the scalar sector of the electroweak theory. In each case, the brief discussion contains only the main points of our more extensive examination of the physics issues.

1. Single Z, W Production

The measurement of the widths (Γ_Z,Γ_W) of these resonances will be especially interesting. The hadronic widths can test perturbative QCD; a precise measurement of Γ_Z can determine the number of kinds of neutrinos. The greatest precision for Γ_Z is likely to come from e^+e^- experiments at SLC and LEP, but there may be advantages to measurements of the W and Z widths in the same experiment. Either the W or Z mass, together with the Fermi constant and the electric charge, determine directly the electroweak angle θ_W, while the W to Z mass ratio determines the ratio ρ of charged to neutral weak current couplings. Very high precision in the measurement of M_W/M_Z provides a sensitive test of the standard electroweak model or departures from it.

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*From information presented to the Subpanel by I. Hinchliffe.

Estimates indicate, that in early running, Tevatron I may yield interesting results on ρ, but cannot determine the number of different neutrinos for which a significant result would require more extended running. Both CBA and DC could attain a precision of a few parts in 10^4 for ρ and a clear count of the neutrinos, the one because of its great luminosity and the other because of its higher energy.

The role of a hadron collider as a $W^\pm$ factory also provides the possibility of studying the charged current couplings of quarks (weak mixing angles) without the theoretical ambiguities inherent in the analysis of decays of hadrons. To the extent that one can identify the primary quark flavor in a hadronic jet, one measures directly the branching ratio $\Gamma(W \to \bar{q}q')/\Gamma(W \to e\nu)$. The most interesting modes will probably be those with a t quark. The principal uncertainty will presumably be in extracting the branching ratio itself from the data, i.e., in identifying the primary flavor of quark jets. If, for example, a high P_T lepton signature is used, there will be a confusion factor among leptons from primary and secondary heavy quark decay, as well as uncertainties associated with the knowledge of the various leptonic branching ratios for heavy quark decay. Initially, accuracy in the branching ratios of the order of five percent should be attempted. Our present knowledge of the mixing angles, although incomplete, is sufficient to permit collider data of that accuracy to indicate substantial mixing with further generations of quarks, as well as total departures from the standard model. At Tevatron I, CBA, and DC, one anticipates sufficient W production for an adequate study of $W^+ \to t\bar{b}$. The higher energy or luminosity machines should produce enough events to permit clean signals to be selected.

2. W^+W^- and $W\gamma$ Production

One of the crucial tests of the electroweak theory is the measurement of the tri-linear couplings characteristic of a non-Abelian gauge theory. The Z°WW coupling, for example, can in principle be measured in the reaction (pp or $\bar{p}p$) $\to W^+W^-X$, and (pp or $\bar{p}p$) $\to \gamma W^\pm X$ could be used to check the prediction for the magnetic moment of the W. At Tevatron I, one would expect to get a sufficient number of W^+W^- events to identify that process. A much higher event rate would be expected at the CBA or the DC, and calculations suggest that a significant test for the presence of the $W^+W^-Z^\circ$ coupling could be performed at either of these facilities. Studies of the W process indicate a good possibility that it could be observed and measured at CBA with the high event rate expected there, and a rough determination of the W magnetic moment could probably be obtained.

3. Jets and QCD

Jet physics, the study of highly collimated clusters of hadrons with a large total p_T, is a promising field of endeavor for hadron colliders. The CERN SPS detectors have, in fact, confirmed the existence of very high p_T jets at rates consistent with expectations. In the context of QCD, the behavior of jet cross sections with P_T should be predictable, and substantial deviations from this behavior at large P_T may signal new physics at small distances. In general, the largest accessible P_T with finite event rate provides the best sensitivity for such investigations. The DC, with its higher energy, could, given its design luminosity and one year of running time, explore out to P_T's of more than 500 GeV. The corresponding reach of both Tevatron I and CBA extends to between 200 and 300 GeV. The increased luminosity of CBA approximately compensates for its lower energy relative to Tevatron I. At lower P_T's, of course, the higher CBA luminosity always provides a higher event rate which allows for a variety of detailed studies using specific triggers. To better quantify the sensitivity to new phenomena at short distances, it is useful to introduce some parameterization for scaling violations; an example is the "compositeness" parameter discussed in a later paragraph.

Complementary to jet studies are QCD probes using leptons and photons. The most incisive tests of QCD and the best measurements of its scale parameter Λ_{QCD} will almost surely be done at electron-proton or e^+e^- facilities. Single γ production in hadron colliders will provide a probe of the dual point-like and hadronic nature of the photon, and the Drell-Yan process will confront the interesting question of factorization in QCD and also probe for new phenomena. A higher event rate than available at Tevatron I is required for the QCD studies.

At CBA, the possibility of high luminosity polarized beams would allow tests of spin symmetries predicted by QCD. It might be that some of these tests are less sensitive to higher order QCD corrections than unpolarized cross sections. Polarization studies might also provide a sensitive probe for new parity-violating interactions among quarks.

4. Heavy Ion Collisions

The CBA is unique among the facilities being considered here in that it could be used as a heavy ion collider. With modifications to the planned injection scheme, heavy ions could be accelerated to energies well above 100 GeV per nucleon. The collisions should then produce, over extended volumes, energy densities much greater than those characteristic of ordinary nuclear matter. Such conditions are of interest to elementary particle physicists because of the insight which might be gained into the interactions among quarks and gluons. Analyses of QCD suggest that both color confinement and chiral symmetry breakdown will disappear under such conditions, producing a "plasma" of gluons and nearly massless quarks interacting weakly. This possible transition is expected to be signaled in a variety of ways, including an increase in strange particle and photon production. The theoretical study of these issues has begun, but considerably more work is called for if relativistic heavy ion collisions are to provide sensitive new tests of QCD. In addition to a possible QCD phase transition, there could, of course, be surprises in store for us if and when these qualitatively new experiments are performed.

5. Heavy Quark and Lepton Searches

Hadron colliders may be sources of heavy quarks through the processes $gg \to Q\overline{Q}$ or $\overline{q}q \to Q\overline{Q}$. The t quark may well have been discovered by the time any U.S. hadron collider will become operational. Its study will point the way towards searches for even heavier generations. It is believed that a high transverse momentum lepton accompanied by a jet plus missing transverse energy should be a clear signature for a semi-leptonic decay of the type $Q \to q + \ell + \nu$. Taking into account branching ratios and necessary cuts of the data, it is estimated that in a year of running at design luminosity a mass range up to M_Q = 100 GeV can be explored by Tevatron I, up to 150 GeV by CBA, and up to 230 GeV by the DC.

All hadron colliders will be copious sources of b and presumably t quarks, which can be studied in their own right once techniques are developed for isolating the events. For example, $B\overline{B}$ production may be isolated by a kinematically constrained decay mode of the B. Flavor-changing neutral currents could be sought in decays like $b \to s\ell\overline{\ell}$ by looking for a charged lepton pair. Some of these studies, as well as similar studies of c quarks, will, of course, be the object of extensive work at SPEAR, CESR, and Tevatron II.

The superiority of e^+e^- machines in heavy lepton searches is above suspicion. However, if hadron colliders that are copious sources of W bosons were to start working some time before LEP and SLC, then they might be exploited to search for heavy leptons with $M_\ell < M_W/2$. Tevatron I, CBA, and DC would all produce enough W's in a year to attempt a search for heavy leptons in this mass range; the CBA, with 3×10^6 $W^+ \to \ell^+$ events per year anticipated, would be the most prolific source.

6. Scalar Sector

An important experimental objective is to discover or exclude the Higgs particle, H, of the minimal electroweak model. *A priori* this requires scanning the entire mass range from 10 GeV to about 1 TeV. Masses below about 40 GeV will be accessible to LEP via the decay $Z^\circ \to H + \ell\overline{\ell}$. In hadron colliders the Higgs particle can be produced together with a W or Z or by gluon fusion via a virtual quark loop. The latter mechanism would be enhanced if there were a further generation of heavy fermions.

If $M_H > 2M_W$, then there is the distinctive decay signature $H \rightarrow W^+W^-$. If the heaviest quark is the t with $M_t < 40$ GeV, the production rate for such a heavy Higgs by either mechanism is too small to be observable at Tevatron I. In fact, estimates show that its detection appears unlikely at any of the intermediate energy colliders, although the DC would have the best chance. If $M_H < 2M_W$, extraction of a signal in a hadron collider appears at present problematic. Barring an ingenious idea, a Higgs in the mass range 40 GeV $< M_H < 2M_W$ may have to await LEP II.

In the technicolor scenario, the most promising candidate for detection among possible particles in the 100-300 GeV mass range appears to be the techni-eta. There are considerable background problems and the high luminosity of CBA seems necessary for a chance of its detection.

7. Heavy Neutral Gauge Bosons

It is often conjectured that the standard electroweak model is incomplete, and that, in particular, there may be heavier neutral gauge bosons, Z', with similar coupling strength to quarks and leptons. SPS collider results suggest that these should be identifiable with a relatively small number of events. Assuming conventional couplings and that 100 produced Z' in 10^7 seconds are sufficient for discovery, Tevatron I can probe masses to about 500 GeV, CBA to 400 GeV, and the DC to 1.2 TeV.

8. Supersymmetry and Compositeness

As an illustrative yardstick of the discovery potential of different facilities, two possible speculative phenomena were considered: supersymmetric partners of ordinary particles ("sparticles") and signals of quark and lepton compositeness. There are supersymmetric models that predict sparticle masses ranging from tens of GeV to several TeV. Only the lightest such object is expected to be stable, but how a given sparticle decays to the lowest sparticle state is again model-dependent. As long as the lightest sparticle is neutral, sparticle production will generally be signaled by missing energy and momentum, but the signal to background ratio, which governs the minimum number of events required for detection, is highly model dependent. If a large number of events is required, luminosity has an advantage over center-of-mass energy. For example, studies within specific model assumptions suggest that 10^4 to 10^5 gluino pair events, depending on the energy, may be required for gluino detection. In this case, Tevatron I could probe masses to about 100 GeV, CBA to 140 GeV, and DC to 235 GeV. If the photino is the lightest sparticle, the subprocess $q + g \rightarrow \tilde{q} + \tilde{\gamma}$ might have a clearer signal; because of the lower cross section, this would require the higher event rate of CBA which could probe a squark mass scale to about 130 GeV if 100 events in 10^7 seconds is a sufficient signal.

Electron-positron colliders can easily probe squark masses up to approximately the beam energy (except at very high energies where statistics are more limited). These facilities are poor gluino producers;

LEP might be sensitive to a gluino mass of 25 GeV or less. An electron-proton facility like HERA could probe sparticle masses to about 100 GeV via e + q production.

The proliferation of "elementary" fermions has led to the conjecture that quarks and leptons are composite, with a compositeness energy scale Λ that roughly reflects the inverse radius of the bound state. One expected effect of compositeness is a form factor correction at each "elementary" fermion vertex parameterized by $1 + q^2/\Lambda^2$; a value $\Lambda = 0.6$ TeV, for example, would enhance the Z° peak by about 10 percent. Another expected effect is the appearance of four fermion contact interactions with an effective Fermi coupling $4\pi/\Lambda^2$ for $\sqrt{s} << \Lambda$. A contact interaction of this type is expected only among fermions with common constituents. Assuming this to be true for left-handed quarks and leptons, Tevatron I can probe values of Λ up to about 2.5 GeV, CBA to 2 TeV and DC to 6 TeV. Away from the Z° pole, electron facilities are generally more sensitive probes of this coupling; for example, an electron-proton facility such as HERA could probe the same quantity to a value of about 7 TeV.

If quarks and leptons have no common constituents, one can look for contact interactions involving only quarks or only electrons. The values of Λ probed in this way are somewhat more limited because the conventional cross sections are higher in both cases, but the conclusion remains that, at moderate energies, electron facilities are superior probes of compositeness, unless quarks have a larger compositeness radius than electrons. In this latter case, CBA has an edge in reach over Tevatron I. At scales beyond 1 TeV, contact interactions are expected to violate parity, and longitudinally polarized beams would be useful. Since right-handed fermions are presumably also composite, including their contact interactions should enhance the overall signal, but the parity violation is expected to be less than maximal. Note that existing e^+e^- data suggest $\Lambda > 0.75$ TeV for electron-electron contact interactions and ν data suggest $\Lambda > 2.5$ TeV for ν-quark interactions.

In composite models one also expects excited states of leptons and quarks. If these are in color multiplets of high dimensionality, their cross sections are increased relative to those for ordinary quark production. If the compositeness scale Λ is very large compared with the mass difference, these states will be quasi-stable. An analysis of the feasibility of identification concluded that both Tevatron I and CBA could detect about 500 events from pair production of decouplet q^*'s of mass 200 GeV using this method, and DC could reach a mass of about 450 GeV.

The above examples indicate that, for a variety of tests of the standard model and for some speculative new phenomena, the CBA is competitive with, but not significantly better than Tevatron I and other facilities (SLC, LEP, HERA) concurrent with it within a few years. The DC would have a larger potential, but not sufficient to probe the TeV region. Should there be new phenomena in the 50-150 GeV mass region, CBA would, of course, be in a strong position to study them.

SECTION VI

EVALUATION OF THE ONGOING PROGRAM

A. Tevatron I Program

According to the present schedule, the Fermilab collider program will furnish 1 TeV x 1 TeV $\bar{p}p$ collisions for physics by 1986-87 with a luminosity $L = 10^{30}$ cm^{-2} sec^{-1}. Two major collision regions are planned: one at B0 where the construction is nearly complete and one at D0 which is not to be finished until 1985. The luminosity in the D0 area may be slightly lower than 10^{30}. A large general purpose detector (CDF) is now under construction for the B0 area. Several proposals have been submitted for the D0 area. It is anticipated that the first round of experiments will run for a period of four to five years with an integrated luminosity of no more than 10^{37} cm^{-2} or 10^{4} nb^{-1} per year of running. The Tevatron I program will divide the available time with the Tevatron II fixed-target program, resulting in 50 percent average utilization of the calendar time for colliding beams.

There are two main features of the Tevatron I program from the point of view of physics opportunities. The primary one is the 2 TeV available for the production of new heavy particle states; another is the fact that the collisions are $\bar{p}p$ rather than pp. The high energy gives Tevatron I a unique position in the world in the 1986-92 time frame. The necessity for the successful construction and operation of a $\bar{p}$ source has the added feature of forcing development of that complex technology in this country.

The anticipated annual yields of standard model intermediate bosons are 2×10^{5} $W^{\pm}$ and 6×10^{4} Z°'s, which can be exploited for detailed study. Examples of the discovery reach of the machine are ~ 400 GeV for new intermediate bosons, ~ 100 GeV for new heavy quarks, and ~ 300 GeV $p_{\perp}$ for QCD jets. Around 1990 this program is anticipated to occupy about 200 U.S. high energy experimental physicists. The viability of the facility after about 1992 will depend on the physics interest and the availability of other facilities. If the level of research activity remains high, then an upgrade of the facility and its detectors may be warranted, with a consequent extension of the useful life of the machine for perhaps another five years.

B. Tevatron II Program

The Fermilab fixed-target program is expected to commence in late 1983. It utilizes 1000 GeV protons on stationary targets to provide approximately a dozen secondary beams. Individual beam lines typically contain more than one experiment; these may operate simultaneously or separately in time, depending on the situation. Many of the beam lines are similar to those that existed during the 400 GeV program, but have been upgraded for higher energy. Several new beams are being constructed, including a new beam dump, a high flux muon beam, a wide-band photon beam, and a polarized proton beam. Although several experiments are continuations of programs begun during the 400 GeV operation, those experiments are undergoing substantial upgrading for operation at the higher energy. In addition, some extensive modifications are being made in light of experience at the lower energy. A rather broad spectrum of physics topics is represented.

Measurements of leptons produced at large transverse momentum have resulted in very interesting comparisons with quark model predictions as well as discovery of the Υ particle. These programs will continue to higher energies and will be broadened to include other beam particles besides protons and other final state particles besides the leptons. Studies of hadron jets and direct photons are also receiving considerable emphasis. Measurements will also be possible in a new polarized proton beam and with polarized targets.

Several experiments are directed toward studies of charm and bottom production, using a variety of novel experimental techniques in a variety of different secondary beams. Of great interest in this regard are efforts to develop vertex detectors and fast on-line processors to extract the events containing decays of heavy quarks. These endeavors should play an important role, complementary to the programs at SPEAR and CESR, in broadening our understanding of the heavy quark spectroscopy and providing measurement of the Kobayashi-Maskawa (K-M) mixing angles.

A program of experiments using neutrino and muon beams will extend the measurements of nucleon structure functions by about a factor of three in Q^2. Given the present confused situation in the Q^2 evolution of structure functions (a smaller value of Λ_{QCD} is obtained when extracted from data at higher Q^2), this extended lever arm in energy may be important. The neutrino experiments will, in addition, be capable of more precise measurements of neutral current couplings which can be compared with measurements from colliders at the Z mass. The muon experiments will study the jet fragmentation and atomic number dependence at very high Q^2. The latter has been of considerable recent interest. Rare phenomena, like multi-lepton production by both muons and neutrinos, will be studied with much higher event rates at higher energies, which will contribute to better understanding of these processes. Holography will be used in the 15-foot bubble chamber to permit direct observation of heavy quark decays spatially separated from their production.

There are many phenomena from lower energies that still defy our understanding. Some of these will also be the subject of Tevatron II experimentation. A high statistics measurement of the electron asymmetry parameter in $\Sigma^-\beta$ decay should corroborate or remove the present large discrepancy between this number as inferred from existing data and the prediction of the Cabibbo model. Precision measurements of CP violation parameters will be performed at levels where effects predicted by certain classes of theories should become manifest. CP violation should be observed in channels not previously seen (e.g. $K^\circ \rightarrow \pi^+\pi^-\pi^\circ$).

A new beam will be constructed consisting of the secondaries remaining after 1000 GeV protons are allowed to interact in a thick target and all strongly and electromagnetically interacting particles are removed (beam dump). The only known particles which should remain are neutrinos. Substantial numbers of ν_τ interactions will be observed in bubble chambers equipped with holographic capabilities to observe directly the path of the produced τ. Beam dump experiments have been performed previously, but none have had the large solid angle, precision detection equipment or high energy planned for the Tevatron II program. Thus, there is substantial room for observation of something new that has not been anticipated.

C. SLAC Linear Collider (SLC)

The SLC is designed to bring e^+ beams with energies up to 50 GeV into collision with e^- beams of equal energies for a total center-of-mass energy of up to 100 GeV in the e^+e^- system. This energy is high enough to produce directly the neutral intermediate boson, the $Z°$, recently discovered at CERN. The exploration of the very rich physics associated with copious $Z°$ production is one of the aims of this project which is now under construction at the Stanford Linear Accelerator Center. The other main goal of the project is to develop the technology of linear e^+e^- colliders which may open the way to much higher energies. The potential advantage of linear colliders lies in the projections that the cost rises linearly with the e^+e^- center-of-mass energy, while the cost of storage ring colliders rises approximately as the square of the e^+e^- energy.

The physics topics to be studied at the SLC include:

1. $Z°$ production and decays: mass, width, decay modes and branching ratios, number of flavors, number of neutrinos;

2. study of top quarks by directly producing the vector $t\bar{t}$ bound state (toponium);

3. search for new particles such as the Higgs, particles predicted by supersymmetry or technicolor, heavy leptons, other new heavy quarks, etc;

4. QCD tests by studying quark and gluon jets; and

5. study of the mixing between the different quark flavors (K-M mixing angles).

SLC thus has objectives similar to those of the LEP machine at CERN. Even though its design luminosity ($\sim 6 \times 10^{30}$ cm^{-2} sec^{-1}) is lower than that of LEP, it is due to have its first physics run about two years earlier (end of 1986). In addition, SLC is capable of collisions with polarized beams, and its very small beam size will allow increased opportunities for close-in vertex detectors and hence, good conditions for the studies of the short-lived particles.

At commissioning, and during the initial stages of operation, the Mark II detector (suitably upgraded) will be used in the single interaction region at the SLC. This has the advantage that physics results can be obtained relatively quickly with a detector that is well understood because of its previous operation at SPEAR and PEP. The plan is to utilize the Mark II detector, appropriately upgraded, during early operation. There are tentative plans to construct a second detector that has been designed expressly for this energy range, so that the full physics potential of the SLC may be eventually exploited.

D. PEP Program

PEP is a high energy e^+e^- storage ring located at SLAC. Since the start of its operation in 1979, the luminosity has been steadily improving and has recently reached a value of 3×10^{31} cm^{-2} sec^{-1}. The parameters of PEP are very similar to those of the PETRA storage ring at DESY in Hamburg, West Germany, and during their initial operation the experiments at both storage rings addressed the same basic physics questions.

During the last year, however, the physics goals pursued at the two installations have somewhat diverged. PETRA has pressed added rf capability to reach the highest energies possible (~ 45 GeV total energy) hoping to exceed the threshold for production of the postulated top quark. On the other hand, PEP has been capitalizing on its recently obtained high luminosity to obtain very high statistics at a single energy (29 GeV total energy).

As far as future upgrades of PEP are concerned, it is the present opinion of the SLAC management that the energy upgrade of the machine is probably not worth the cost unless the top quark is found to be accessible. A total energy of 43 GeV could be reached at an estimated cost of \$20 million to \$24 million. There are plans, however, to upgrade the luminosity even further, both by μ-β insertions as well as adiabatic improvements. This luminosity increase might necessitate detector improvements at the level of about \$1 million per detector to enable them to function in the higher background environment. In light of the excellent performance of the CESR and DORIS storage rings, there are no plans for PEP to run in the 10 GeV Υ region.

The physics topics studied at PEP are quite diverse. In the field of hadronic physics, the emphasis is on detailed QCD tests by looking at the quark and gluon jets, flavor correlations in 2 jet annihilation, and studies of the hadronization process for both the light and heavy quarks. In the electroweak sector, there are measurements of the neutral weak coupling constants of quarks and leptons by studying the forward-backward asymmetry and determinations of the weak mixing angles from B meson decays. Recent upgrades of several detectors with precision vertex chambers will allow precise determination of lifetimes of τ leptons, charmed states, and conceivably even the b quark states. In addition there is an extensive two-photon physics program and a recently approved dedicated experiment to search for the photino, the photon partner in the supersymmetric theories.

At present there are five general purpose detectors operating at PEP, namely, Mark II, High Resolution Spectrometer (HRS), a detector oriented around a Time Projection Chamber (TPC), a magnetic calorimeter detector (MAC), and (DELCO) a detector oriented around a large threshold Cerenkov counter. In addition a specialized detector, covering full 4π solid angle, has been recently approved for the sixth interaction region to look for missing energy and transverse momentum to search for possible long-lived non-interacting neutral particles postulated in supersymmatic theories. Two specialized experiments have been already completed: a free quark search and a monopole search, both with negative results.

The Mark II detector is slated to move to SLC in approximately two years. Some of the members of the HRS collaboration have expressed interest to move that detector to the TRISTAN storage ring after two more years of data taking at PEP. It appears that two of the PEP interaction regions may be empty starting in FY 1986 since, at this time, no proposals have been received for a new initiative to be carried out in those regions. Because of the recent significant improvements in luminosity, the PEP program can look ahead to several years of vigorous and productive physics research. However, unless the luminosity continues to increase, the machine may be phased out in four to five years.

E. SPEAR Program

Even though this medium energy storage ring has been operating for over a decade, its present physics program continues to be very productive. The main value of SPEAR rests in the fact that it is the unique e^+e^- facility in the world in the 3-5 GeV total energy region. This energy region allows one not only to study the charmonium spectroscopy, but also to study production and decay of various charmed states in relatively clean circumstances. However, study of differing physics topics (ψ' decay modes, D mesons, F mesons, charmed baryons) necessitates running the machine at different energies and thus these studies cannot be performed concurrently.

SPEAR has two interaction regions, but since the removal of the Crystal Ball detector to DORIS, only one of the regions has been occupied. It serves a general purpose detector known as Mark III. This detector has been optimized for study of the SPEAR energy region and emphasizes good efficiency for low energy gamma rays, precision time-of-flight system to separate π's from K's and protons over most of the kinematic range, and good charged particle momentum measurement. The detector has large solid-angle coverage and can be used to reconstruct a large fraction of charmed particle decays.

At present, no proposals have been received for an experiment in the second interaction region. Recently, SPEAR has been running half time as a synchrotron radiation source for the Stanford Synchrotron Radiation Laboratory and the plans are to continue that mode of operation in the foreseeable future. It is anticipated that the Mark III program will remain productive at least until 1988.

F. SLAC Fixed-Target Program

Since the start of its operation in 1966, the fixed-target experimental program at SLAC has contributed greatly to the elucidation of the laws of physics. It gave rise to such pioneering experiments as electron-nucleon inelastic scattering that gave the first dynamic evidence for quark and gluon composition of the nucleons, and the parity violation in electron nucleus scattering that helped to establish the validity of the Glashow-Weinberg-Salam model. In addition, the program also included extensive photoproduction experiments and a whole spectrum of experiments with secondary hadron beams which studied the hadron spectroscopy and weak interactions in K meson decays.

The physics program has been winding down in recent years, and the main function of the linear accelerator has been to serve as an injector for both the SPEAR and PEP storage rings. The latest experiments performed were an axion search and a charmed particle lifetime measurement in the SLAC Hybrid Bubble Chamber facility. The latter experiment is scheduled to conclude during this fiscal year or the beginning of 1984 and no further bubble chamber experiments are planned at the present time. A new experiment, designed to study the scattering of electrons on nuclei and, thus, elucidate some of the recently observed anomalies in these processes, has been recently approved, is currently running, and is scheduled to be completed this summer. Another experiment to study electron-nucleon scattering at high Q^2 is due to run at the beginning of calendar year 1984.

The energy of the accelerator is being upgraded at the present time to 50 GeV in connection with the SLC program. Even though there are no long range plans at the present time to continue the fixed-target program, this activity could be reviewed in the future should there be renewed physics interest. In addition, an off-axis injector will be constructed shortly to give a relatively cheap way of generating 3-4 GeV electrons. This improvement program is motivated mainly by the proposed nuclear physics program but will also provide an economical way of filling SPEAR.

G. CESR Program

Since CESR came into operation in late 1979, the CLEO and CUSB experiments have located the first four ^3S states and first two triplets of ^3P states in the $b\bar{b}$ quarkonium system. They have measured the dilepton and hadron decay widths of the ^3S upsilons as well as the widths for $\mu\mu$ transitions among them and the radiative transitions involving the ^3P states. The fourth upsilon has served as a "B meson factory", allowing studies of the weak decays of the b quark: semileptonic branching ratios, multiplicities, inclusive K, D, p, and Λ yields, and exclusive modes. The results have confirmed the standard six-quark model and have established the $b \to c \to s$ cascade as the dominant decay mode.

This is only a beginning, however. An important gap in the $b\bar{b}$ bound state spectroscopy is the ^1S η_b states which should be accessible from the upsilon states by a sequence of hadronic and radiative transitions. The systematic reconstruction of upsilon decay modes promises possible discoveries of glueball, Higgs, axions, supersymmetric partners, and perhaps other surprises. A thorough study of B decays will be required to measure the generalized Cabibbo weak mixing angles, $B^\circ B^\circ$ mixing, CP violation, and rare modes such as $B \rightarrow \tau\nu$. The higher mass b-flavored hadrons, B^*, B_s, Λ_b etc., may yet be seen.

The studies of the b quark at CESR are important for progress in our understanding of both strong and weak interactions. Because the b quark is so massive, the $b\bar{b}$ system is especially amenable to quantitative QCD predictions. The weak decays of the b quark are unique in that they permit measurement of branching ratios which depend significantly on the weak mixing angles other than the original Cabibbo angle; the $\bar{B}^\circ B^\circ$ system (or $\bar{B}_s^\circ B_s^\circ$) may be our only chance to observe CP violation outside of the $\bar{K}^\circ K^\circ$ system. One can hope that this work will lead to insights as to why, or whether, there are three generations of quarks and leptons.

The future CESR physics program requires significant increases in quantity and quality of data for the following reasons. Relative to cross sections in the charmonium region, $b\bar{b}$ cross sections are suppressed because of quark charge and because of increased energy. Moreover, in the $b\bar{b}$ case there are more states and more decay modes and, therefore, smaller branching ratios. The CESR luminosity improvement program is needed to provide at least a ten-fold increase in rates.

The CLEO group is undertaking an extensive upgrade of the detector to provide significantly improved charged tracking and particle identification as well as photon energy resolution. There are plans to upgrade the North experimental area and to replace the present CUSB experiment by an improved detector emphasizing very high quality photon and lepton detection.

H. Fixed-Target Program at BNL

The fixed-target experimental program at BNL is based on the 33 GeV proton synchrotron, the AGS, which provides not only intense external beams of high energy protons but also secondary separated beams of pions and kaons and low energy neutrino beams. Many major discoveries have been made with the AGS throughout its distinguished history. Some of the highlights are: two neutrinos; the "Omega Minus" hyperon; CP violation; the J particle; and the charmed baryon. The physics program now focuses on: studies of the weak interaction via neutrino interactions and K meson decays; the search for neutrino oscillations; hadronic interactions via spectroscopy, dynamics and polarization studies; QED via measurements of muonic atoms with helium nuclei; and a strong program of intermediate energy nuclear physics studying kaon-hypernuclei and K^--and hyperon-induced atomic X-ray emission.

The AGS is the unique proton accelerator in the world in this energy range and provides the highest average proton intensity available of any proton machine. It provides up to 10^{13} protons per pulse every 1.2 seconds for neutrino physics and, with a one second flattop, every 2.5 seconds for electronic counter experiments.

At present, there is an intense interest in neutrino oscillations and, as yet, unobserved rare K decays. The eventual future of the AGS as a physics tool will be to a large extent determined by the results of the experiments forthcoming in the next three or four years.

I. Los Alamos Meson Physics Facility (LAMPF)

Even though LAMPF is supported as part of the medium energy physics program, there are at LAMPF a variety of particle physics activities centered around a program of precision decay experiments and neutrino interactions. A comprehensive search for lepton number violating μ decays at the 10^{11} level exploiting a 28 MeV/c, 100 percent polarized muon beam is under way. The modes $\mu \rightarrow 3e$, $\mu \rightarrow e\gamma$, and $\mu \rightarrow e\gamma\gamma$ are all being sought, and the sensitivity is expected to improve to the 10^{13} level over the next several years. A high statistics study of normal μ decay is under way as a precision test of the "standard model."

An experiment to measure $\nu_e e$ scattering is under way. This is sensitive to the postulated (destructive) interference between the charged and neutral currents. An experiment to study $\nu_\mu \rightarrow \nu_e$ oscillations is in the construction phase at the present time. The proposed Los Alamos Neutrino Facility would allow the performance of additional oscillation experiments sensitive to very small mixing angles.

J. Non-Accelerator Program

The U.S. high energy physics budget has traditionally supported a modest and diversified program of non-accelerator particle physics. Recent predictions from grand unified theories have inspired a new intense interest in searches for nucleon instability, heavy primordial magnetic monopoles, neutron oscillations, and other new phenomena. A stimulating merging of the interests of traditional particle physicists with those of cosmic ray physicists, astrophysicists, astronomers, and cosmologists is taking place. A list (ordered by energy scale) of non-accelerator experiments in these areas includes the following:

- Searches for gravity radiation

- Solar neutrino measurements

- Searches for neutrinoless double beta decay

- Neutrino mass searches

- Searches for neutrinos from gravitational collapse

- Neutron-antineutron oscillation searches
- Proton decay searches
- Studies of atmospheric neutrinos and neutrino oscillations
- Studies of extraterrestrial and extragalactic cosmic rays
- Gamma-ray and neutrino astronomy up to several TeV in energy
- Searches for magnetic monopoles

It is commendable that there is at least some U.S. effort in all of these areas. These experiments add significantly to the diversity of the U.S. high energy physics program and are especially desirable in the present theoretical climate where seeing what lies beyond the standard model is of prime importance. Most of the experiments listed above have direct relevance to the program of elementary particle studies at high energy accelerators. Indeed, some of these experiments explore physics on an energy scale beyond that attainable with accelerators. Moreover, constraints on new theoretical ideas often come from experiments whose initial objectives were of an entirely different purpose. For example, measurements in X-ray astronomy put constraints on monopole catalysis of nucleon decay; solar neutrino flux measurements limit the rate of monopole catalysis in the sun.

Among the better known efforts, one should mention the proton decay program which, during the last few years, has been especially strong and diversified. The recent results from the large water Cherenkov experiment (Irvine-Michigan-Brookhaven collaboration) have already put in question some of the simplest grand unified theories and refined measurements continue to come from new data. A second water Cherenkov detector (Harvard-Purdue-Wisconsin) using a somewhat different technique is just coming into operation. The future tracking calorimeter experiment (Soudan II) should provide information that is complementary to that obtained from the water Cherenkov technique.

The results of the solar neutrino experiment have provided a longstanding puzzle in the world of physics. Its resolution may come from better understanding of the elementary particle area or from the field of astrophysics, to mention only two possibilities. The reactor neutrino experiments provide a unique means of probing the questions of neutrino masses and lepton mixing, because of the high fluxes and low energies of reactor neutrinos.

It is very likely that during the next decade the non-accelerator elementary particle program will continue to play a vital and complementary role to the accelerator-based research. It not only provides a means of achieving an energy scale inaccessible to present day accelerators, but also frequently connects the field of high energy physics with many of the other branches of physics.

K. Research by U.S. High Energy Physicists Abroad

The field of high energy physics has always involved a great deal of international collaboration. In the late 1950's and early 1960's when the U.S. facilities and their level of support were superior to what was available in the rest of the world, there was a great deal of individual and institutional participation by foreign scientists in research work performed at U.S. laboratories, but relatively few U.S. physicists worked abroad. The situation became more balanced in the late 1960's and early 1970's with the growth of the CERN and DESY laboratories in Europe. Furthermore, the high energy facilities became more expensive so that frequently it was not practical, nor necessary, to duplicate in this country (or abroad) an accelerator or storage ring that already existed elsewhere. The ISR at CERN and the linear accelerator at SLAC are two primary examples of such uniqueness. The ISR was the first foreign facility with extensive participation by U.S. physicists in its research program.

During the last decade the pendulum has swung even further in this direction. A much more vigorous facility construction program in Europe, coupled with a higher level of support, resulted in a situation where an increasing number of the U.S. high energy physics community started to look abroad for their research opportunities. During the past ten years there has been an extensive U.S. institutional involvement in both the ISR at CERN and PETRA at DESY. There is at present a limited U.S. participation in the $\bar{p}p$ program at the S$\bar{p}$PS collider. Looking into the future, there is a strong commitment by many U.S. groups to work at LEP and a significant interest expressed by others in doing experiments at HERA and at TRISTAN in Japan. Other European facilities that have U.S. participation are the DORIS storage ring at DESY and the low energy $\bar{p}p$ ring at CERN, LEAR.

Frequently, the attraction of foreign facilities is their unique nature in the world (e.g. ISR, HERA, LEAR, TRISTAN), and hence, a unique physics capability. Alternatively, the attraction might have been a turn-on date that was earlier (e.g. PETRA, S$\bar{p}$PS collider) than the date for an equivalent facility in this country. Finally, the European facilities frequently provide a better level of support and/or a larger number of opportunities (e.g. LEP) than the American equivalents.

The increasing involvement of U.S. physicists in research abroad can be seen quantitatively by looking at the fraction of the university high energy physics budget that is spent for work outside this country. That fraction has risen from 8 percent in FY 1982 to an estimated 12 percent in FY 1983. The fraction will probably rise even further in FY 1984 in connection with the increasing expenditures for the LEP detectors.

There is no doubt that progress in science benefits from a wide exchange of ideas. Furthermore, because of the large cost of most of the high energy physics facilities, there is an economic necessity in the field for a large level of international collaboration and a mutual exchange of research opportunities. However, there is a strong feeling in the high energy physics community that, at the present time, the situation has become somewhat one-sided and the U.S. effort in providing frontier opportunities has not kept pace with the rest of the world.

SECTION VII

INTERNATIONAL COOPERATION IN HIGH ENERGY PHYSICS

Science generally, and high energy physics in particular, benefits from the widest possible free exchange of ideas, information, and experience. The support by many countries of high energy physics as an important, fundamental area of research means that the high energy physics community is truly international in nature. Exchanges of ideas, results, and experience occur through international conferences, publications, and visits, and are even more effectively realized when physicists work for extended periods in the institutions of other regions. Scientific research is enhanced, with the added benefit of broad intellectual and cultural exchanges.

International collaborations permit for each nation a more diversified program of physics than is possible with its own accelerator facilities. As the size and cost of the accelerators increase, the international availability of each facility becomes more desirable. Provided the exchanges go in both directions, increased costs for any one country are not appreciable, yet the benefits can be shared by all. The alternative is a narrowly focussed program in each country and a resulting diminution in overall effectiveness of the research.

For many years there has been international collaboration on an experiment-by-experiment basis at facilities in the U.S. and in Europe. For the past 5 years the U.S. and Japan have had a formal agreement on cooperation in high energy physics with Japanese and U.S. physicists collaborating on experiments and R&D projects at the four U.S. accelerator laboratories and LBL. Similarly, there is a long-standing US-USSR program of collaboration on the fundamental properties of matter. Of more recent origin is the collaborative program between the U.S. and the People's Republic of China, bringing scientists and engineers to the U.S. for extended periods of study, research, and accelerator design.

Some years ago, the high energy physics community realized that eventually the required facilities would be beyond the capabilities of any one nation and established the International Committee for Future Accelerators (ICFA). The Committee has met regularly in the past and continues its planning and coordination functions with U.S., U.S.S.R., Western European, and Japanese participation.

In the area of international cooperation in high energy physics, the Subpanel makes the following comments:

1. Laboratories and funding agencies should continue to support participation of individuals and groups in experiments at facilities available abroad, provided the physics goals warrant it. This form of international cooperation has a long tradition. In the U.S., the level of support for experiments done abroad is presently about 10 percent of the program funded through the universities. As the effort grows with participation in the LEP project and other foreign facilities, the DOE and NSF will need to examine carefully proposals for research abroad but be responsive to excellent physics opportunities wherever they occur.

2. Duplication of facilities with essentially the same capabilities becomes unacceptable as size, complexity and cost increase. It is, therefore, natural to contemplate U.S. participation in outstanding ventures at foreign laboratories in order to provide greater physics opportunities for the U.S. and the other countries. On the other side, the U.S. should welcome foreign involvement in exploitation of new U.S. facilities, in either a general way or for a specific capability.

APPENDIX A

CHARGE

1983 HEPAP SUBPANEL ON NEW FACILITIES

Consider and make recommendations relative to the scientific requirements and opportunities for a forefront United States High Energy Physics Program in the next five to ten years. Make specific recommendations with respect to possible new construction items for FY 1985. In its deliberations the Subpanel should consider opportunities that may be available later as it weighs its specific recommendations. The report of the Subpanel should include a definite recommendation concerning the proposed Colliding Beam Accelerator at Brookhaven National Laboratory. The Subpanel should estimate the program funding required to realize the scientific opportunities associated with the recommendations, and should give the relative priorities of the actions it recommends.

APPENDIX B

Membership and Consultants

1983 HEPAP Subpanel on New Facilities

Membership

S. Wojcicki, Chairman - Stanford University
* J. Adams, - CERN
T. Appelquist - Yale University
C. Baltay - Columbia University
M. Gaillard - U. California, Berkeley/Lawrence Berkeley Lab.
J. Jackson - U. California, Berkeley/Lawrence Berkeley Lab.
D. Keefe - Lawrence Berkeley Lab.
A. Kerman - Mass. Inst. of Tech.
L. Pondrom - University of Wisconsin
J. Rees - Stanford Linear Accelerator Center
* C. Rubbia - Harvard University/CERN
F. Sciulli - Columbia University
M. Tigner - Cornell University
S. Treiman - Princeton University
J. Vander Velde - University of Michigan
H. Williams - University of Pennsylvania
B. Winstein - University of Chicago
E. Fowler, Executive Secretary - Department of Energy

Technical Consultants

D. Hartill - Cornell University
R. Lundy - Fermi National Accelerator Lab.
R. Neal - Stanford Linear Accelerator Center
R. Palmer - Brookhaven National Lab.
C. Taylor - Lawrence Berkeley Lab.

* The two European members of the Subpanel took a full part in the discussions at the meetings and in reaching the recommendations but they are not to be associated with those remarks in the report concerning the relative position of U.S. high energy particle physics in the world and the attitudes expressed about international relationships.

APPENDIX C

MANPOWER

The Subpanel considered the question of the distribution of activities at the new or upgraded facilities in the late 1980's. Specifically it looked at the intentions of each research group in the country, where a research "group" was (loosely) defined as a "task" as funded by the DOE, or as an NSF-supported effort. In the latter case, the different research efforts under one large (umbrella) contract were considered as individual groups. Research groups which are based at the national laboratories were also included.

The Subpanel identified about 180 groups in the country which are doing or have traditionally done accelerator-based high energy physics experimental research.

Primarily on the basis of the approved programs at the various laboratories, the Subpanel estimates that in the late 1980's about 40 percent of the research groups in the country will be working at Fermilab (28 percent at Tevatron II and 12 percent at Tevatron I), 11 percent at SLC, 5 percent at CESR, and 12 percent abroad (9 percent at LEP and 3 percent at TRISTAN). The remainder of the groups (30 percent) are, at this time, uncommitted to any of the above programs. The currently uncommitted groups comprise about 170 Ph.D. physicists which corresponds to 12 percent of the total high energy physics experimental Ph.D. physicists in the country. Some of these groups could no doubt be involved in programs not included in the above tabulations, namely, AGS, SPEAR, PEP, HERA, non-accelerator physics, and detector development for the SSC.

APPENDIX D

FUNDING REQUIREMENTS FOR THE RECOMMENDED PROGRAM

The funding requirements of the program recommended by the Subpanel can be broken down into three main components: vigorous support of the ongoing program, the expeditious completion of the already-approved construction projects, and the pursuit of the Superconducting Super Collider project on a very rapid time scale.

1. The ongoing program support consists of the funding for the operation, equipment, AIP and GPP at Fermilab, Brookhaven, and SLAC and the university program by the DOE, the funding of CESR at Cornell, and a portion of the university program by the NSF. The university program for the sake of the present discussion includes the funding of U.S. theoretical groups, the experimental work by U.S. groups at U.S. and non U.S. accelerators as well as non-accelerator experiments, and the two non-accelerator laboratories, LBL and ANL. In FY 1983 the support of this ongoing program is $386 million (FY 1983 dollars) by the DOE and $30 million (FY 1983 dollars) by the NSF. We estimate that a level of ~$400 million (FY 1983 dollars) is the minimum for the DOE support of the operations and equipment costs for a healthy and vigorous program, not including construction, but including an active level of advanced accelerator R&D in the years ahead. An increased level of NSF funding will be required to allow the recommended upgrade and full utilization of the CESR program at Cornell.

2. In addition to the above, the approved construction projects, Tevatron II, Tevatron I, and SLC require a total of approximately $40 million, $60 million, and $100 million, respectively, (all in FY 1983 dollars) over the years FY 1983 to FY 1986 for completion.

3. The rapid pursuit of the SSC project will require a total R&D budget of approximately $150 million (FY 1983 dollars) spread out over the next four to five years beginning in FY 1984. The construction costs of this project are not well known at this time, but the goal of the R&D phase of the project is to reduce these costs to under $2 billion (FY 1983 dollars). This cost would be distributed over an expected construction period of five to six years.

4. In summary, the funding requirements for the recommended DOE program are roughly $500 million (FY 1983 dollars) annually for the next four or five years while the SSC is in the R&D phase. An increased NSF support of Cornell will also be required. An increase to a total funding level of $750 million to $800 million (FY 1983 dollars) annually for the period of SSC construction (during which the annual funding rate would be similiar to that during the peak of the construction of the Fermilab 400 GeV accelerator) will be required from the DOE. After this construction period, the funding level can be substantially reduced, perhaps returning to the vicinity of the present level of operation.

REPORT OF THE SUBPANEL ON
LONG RANGE PLANNING FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM
OF THE U. S. DEPARTMENT OF ENERGY HIGH ENERGY PHYSICS ADVISORY PANEL

JANUARY 1982

Department of Energy
Washington, D.C. 20545

HIGH ENERGY PHYSICS ADVISORY PANEL

February 26, 1982

Dr. Alvin Trivelpiece
Director, Office of Energy Research
U.S. Department of Energy
Washington, D. C. 20585

Dear Al,

I am transmitting to you the Final Report of the HEPAP Subpanel on Long Range Planning for the U.S. High Energy Physics Program, chaired by Dr. George Trilling of U.C.-Berkeley. HEPAP discussed this report at its meeting at DOE Headquarters in Germantown, Maryland, on February 19-20, 1982, and supports its recommendations. This Subpanel report completes the most extensive study of the national High Energy Physics Program of recent years. It presents the Department of Energy with a valuable analysis of the scientific prospects and of the technical status and opportunities; and, on behalf of HEPAP, I wish to thank the chairman, Dr. Trilling and the Subpanel members for their arduous and important labors.

As stated by the Subpanel: "The future of high energy physics promises to be as exciting and productive as its profoundly successful recent past. Whereas the U.S. program has dominated this field until recently, the importance of high energy physics has stimulated vigorous competition from other regions of the world." Looking ahead at the 1980's and into the 1990's and facing the challenge of maintaining a vital and eminent program, the strategic question in U.S. high energy physics is the balance between utilization of existing facilities, the construction of the major facilities for the next stage of research, and the advanced accelerator research and development effort necessary for the long-range progress of the field. The problems of formulating a successful policy are difficult and complex, and the three major program elements mentioned above are often coupled in the consideration of specific projects.

The Subpanel has arrived at a set of recommendations which it believes will provide the necessary ingredients for a long-term vital and competitive U.S. program. This program requires an annual support which rises to about $440M DOE and $35M NSF (FY 1982 dollars) ($480M and $38M respectively in FY 1983 dollars) by 1984 or 1985 at the latest.

Its key elements are:

Construction of ISABELLE at Brookhaven with completion by the end of this decade.

Adequate utilization and maintenance of existing accelerator and storage ring facilities, and support of important non-accelerator particle physics projects.

Expeditious completion of the Energy Saver at Fermilab.

Implementation of the Tevatron II (TeV II) fixed-target program at Fermilab, in all three experimental areas.

Completion of the Tevatron I (TeV I) proton-antiproton collider at Fermilab, including the provision of a major detector facility.

Continuation of the major R&D effort at SLAC on the Stanford Linear Collider (SLC), with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for expanded physics research.

Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating RF structures, superconducting RF cavities, and novel means of acceleration.

HEPAP strongly endorses this recommendation as providing the essential elements for a healthy, albeit lean U.S. program in high energy physics. It provides for a level of effort about the same as the 1978 DOE/OMB Agreement and permits effective utilization of present facilities, construction of the new facilities required by the end of the decade, and advanced technology R&D essential for the program future. Although well below the support level of the Western European program the level of the DOE/OMB Agreement would still allow us to compete effectively on a world basis. HEPAP urges that the annual budgets for high energy physics be restored to the level required for this balanced program.

To respond to the eventuality that the above level is not reached in the near future, the Subpanel carefully studied the program at a continued budget level equal to $395/34M in FY 1982 dollars. This is close to the level in the President's proposed FY 1983 budget ($429M for DOE). It is about 9 percent below the budget level of the 1978 Agreement between OMB and DOE which identified the minimum level under which the construction of ISABELLE and of the Tevatron project could be initiated. At the $395/34M level the budgetary impact on the other essential elements of the program of continuing the present ISABELLE project is severe. If the budget remains no higher than this level, the Subpanel recommends and HEPAP concurs that this construction project cannot continue.

The Subpanel concluded that it is essential for an active and healthy U.S. program in the early 1990's that a major new facility for experimentation be available by the end of this decade. If the present ISABELLE project must be discontinued, then another major facility must be started in the mid-1980's so as to be available for research by 1990. This facility would have to be less expensive than ISABELLE but must be capable of supporting a broad physics program and of exploring new frontiers. HEPAP strongly agrees that such a new facility is essential by the end of the decade. By that time most of the present facilities will be past their peak usefulness and unable to meet the requirements of many forefront experiments. Thus a major new project is clearly required if the facilities available to U.S. high energy physicists in the early 1990's are to be competitive and adequate to the intrinsic potential of the field.

The Subpanel has recommended that the linear collider project SLC, the major R&D effort at SLAC, proceed so that the R&D phase, including construction of necessary conventional facilities, can be completed in 1986. HEPAP strongly endorses this recommendation. The SLC was discussed at length and HEPAP wishes to comment here briefly on its significance to the U.S. program. The SLC is an accelerator research project which is central to the long term development of a very high energy electron-positron collider, and at the same time offers the opportunity to do some very exciting physics in a timely fashion. It is this latter point which drives the schedule. The SLC is not the new major facility for the 1990's recommended by the Subpanel and endorsed by HEPAP; it is an example of the type of relatively large R&D project which is now necessary in the development of new accelerator systems. Viewed in the light of the cost, complexity, and scientific importance of a future very high energy linear collider, the SLC is a reasonably scaled construction effort for R&D purposes, and is strongly supported by HEPAP.

HEPAP strongly emphasizes the importance of maintaining the budget at a level at least equal to that currently requested for FY 1983 ($429M for DOE). This budget level will permit important improvement in FY 1983 of the currently poor utilization and inadequate support of existing facilities, and also, expeditious completion of the Fermilab Energy Saver. HEPAP concurs with the Subpanel assignment of highest immediate priority to these two program elements. At this budget level the U.S. program will be competitive and strong through the 1980's. However, HEPAP also reemphasizes the importance of a major new facility so that we can envisage a strong and vigorous national high energy program in the 1990's.

Sincerely,

Sid

Sidney D. Drell
Chairman, HEPAP

50th
ANNIVERSARY 1931-1981
Lawrence Berkeley Laboratory
University of California • Berkeley, California 94720

February 10, 1982

Professor Sidney Drell, Chairman
High Energy Physics Advisory Panel
Stanford Linear Accelerator Center
P. O. Box 4349
Stanford, CA 94305

Dear Sid:

I am submitting herewith the Final Report of the 1981 - 1982 HEPAP Subpanel on Long Range Planning in High Energy Physics. I sincerely hope that this Report will be of some value to HEPAP, to the Department of Energy, and to the National Science Foundation as they chart the future course of the national high energy physics program.

Sincerely,

George H. Trilling

George H. Trilling

Enclosure

GHT:cfd

SECTION I

PREFACE

The High Energy Physics Advisory Panel (HEPAP) Subpanel on Long Range Planning was formed in August 1981 to advise on the U.S. High Energy Physics Program. The Charge to the Subpanel is given in Appendix A, and the membership is listed in Appendix B.

The Subpanel held an initial organizational meeting on September 4-5, 1981, in Washington, D.C. On September 24-26, 1981, the Subpanel met at DOE Headquarters in Germantown, Maryland, and heard presentations from representatives of the high energy accelerator laboratories--Cornell, SLAC, Fermilab, and Brookhaven--and from various other individuals. The Subpanel has since held further meetings on October 9-10, 1981, at Brookhaven, on October 17-19, 1981, at DOE Headquarters in Germantown, Maryland, on December 2-5, 1981 in Napa, California and on January 14-17, 1982, at NSF Headquarters in Washington, D.C. A Technical Subgroup of the Subpanel supplemented with various consultants made further visits to Fermilab (October 1-2); to Brookhaven (October 8-9), to SLAC (November 3-4) and to Cornell (November 19-20) to assess the technical status as well as the anticipated schedule and costs for the Tevatron I (TeV I) project at Fermilab, the ISABELLE project at Brookhaven, the Stanford Linear Collider (SLC) project at SLAC, and the CESR II project at Cornell. The list of Subpanel members and consultants who participated in these visits is given in Appendix B. To solicit input from the high energy community, a letter from the Subpanel Chairman inviting such input was sent to each member of the Division of Particles and Fields of the American Physical Society. About 170 responses to this invitation, representing the views of 240 people, were received and distributed to members of the Subpanel.

The present Subpanel is the fifth in a series of recent Subpanels (1974, 1975, 1977, 1980) which have reviewed the status of the U.S. High Energy Physics Program and made recommendations for future directions. Both the ISABELLE project and the colliding beam effort at Fermilab were recommended by the 1977 Subpanel. The anticipated funding level on which these recommendations were based was higher than the highest level of the present Charge; namely, 10 percent above the DOE/OMB Agreement of 1978. However, the 1977 Subpanel did consider a lower level, closely equivalent to the intermediate level of the present Charge (the DOE/OMB Agreement of 1978), still recommended the ISABELLE project, but stated that "this funding level on a long term basis is close to that which would require a drastic revision in the long range program and probably is not adequate to keep the U.S. in a world competitive position in this field."

As requested by the Charge, an Interim Report was submitted on November 1, 1981, evaluating the ISABELLE priority for various constraint levels. The basic conclusion of the Interim Report can be summarized by repeating here the recommendation given therein:

> "The Subpanel strongly recommends that the ISABELLE project go forward and be completed during this decade. ISABELLE provides a major new facility that will enable the U.S. High Energy Physics Program to remain

> active and healthy during this decade and into the next. ISABELLE construction and the other essential components of the U.S. Program require a minimum annual level of support that averages $440M (FY 1982 dollars) in DOE funds per year. If support at this level cannot be made available in time for ISABELLE completion within this decade, the ISABELLE project cannot be continued."

The Interim Report was presented to HEPAP at a meeting held November 1, 1981, in Washington, D.C. and was unanimously endorsed.

The present document is the Final Report of the Subpanel. In its long-range planning recommendations, the Subpanel has considered in detail two specific yearly budget levels; namely, (1) $440M DOE and $35M NSF, and (2) $395M DOE and $34M NSF (FY 1982 dollars). These are respectively 4 percent above and 8 percent below the DOE/OMB Agreement of 1978. It has also addressed the impact of a lower budget level and identified some of the desirable programs which could not be supported even at the higher of the above levels.

The organization of this Report is as follows. Section II contains a Summary of the Subpanel's planning recommendations. Section III has a discussion of the Scientific Issues. Section IV provides a summary of facilities in High Energy Physics, including those in operation, those under construction and those proposed. Section V provides Technical Evaluation of facility construction and development projects at the U.S. accelerator laboratories. Section VI discusses that part of the elementary particle physics program not based on accelerators (Non-Accelerator Physics). Section VII discusses in detail the proposed high energy programs at the various budget levels (Facility Recommendations and Long Range Plans). Section VIII deals with the area of international collaborations and Section IX discusses ways of improving the long-range planning process itself.

SECTION II

SUMMARY

It is very likely that history will show the decade of the 1970's to have been one of the greatest periods of progress in our understanding of the basic constituents of matter and the forces acting between them. This progress was made possible by a variety of crucial experimental discoveries and theoretical contributions in which U.S. high energy physicists played the major role. The future of high energy physics promises to be as exciting and productive as its profoundly successful recent past.

The U.S. High Energy Program remains strong, but it faces vigorous competition from other regions of the world. To maintain its vitality and preeminence over the next decade it requires the following major ingredients: (1) strong exploitation of existing facilities; (2) the expeditious completion of construction projects which will expand these facilities over the next few years; (3) the construction of a substantial new facility to be ready for research by the end of the 1980's; and (4) the vigorous pursuit of a wide range of advanced accelerator R&D programs in preparation for the design and construction of a higher energy accelerator which would probably be initiated near the end of this decade.

The Subpanel has considered how best to accomplish these goals under two different budgetary assumptions; namely, average yearly support levels of $440M DOE, $35M NSF, and $395M DOE, $34M NSF (FY 1982 dollars). It has also considered the impact of a yet lower support level of $360M DOE and $32M NSF. Its recommendations for the highest of these levels (assumed to be reached from the present lower levels by FY 1984 or at the latest FY 1985) are as follows (no priority order is implied):

--Construction of ISABELLE at Brookhaven with completion by the end of this decade.

--Adequate utilization and maintenance of existing accelerator and storage ring facilities, and support of important non-accelerator particle physics projects.

--Expeditious completion of the Energy Saver at Fermilab.

--Implementation of the Tevatron II fixed-target program at Fermilab in all three experimental areas.

--Completion of the Tevatron I proton-antiproton collider at Fermilab, including the provision of a major detector facility.

--Continuation of the major R&D effort at SLAC on the Stanford Linear Collider, with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in

FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for physics research.

--Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating structures, superconducting RF cavities and novel means of acceleration.

This program, although lean in many respects, enables the U.S. to maintain an active and healthy position in the field through the end of this decade and into the 1990's.

For the lower budget level of $395M DOE and $34M NSF, the program differs in that the present ISABELLE project cannot continue*, but is replaced by the following recommended element:

--With the present ISABELLE project terminated, a start by the mid-1980's on a new high energy construction project of more modest financial scope. Although it is premature to recommend a specific direction at present, the Subpanel is aware of a number of interesting concepts currently being considered by the community and urges intensified discussion, development, and R&D aimed at selection and authorization by the middle of the decade.

Such a facility would have to be lower in cost than the ISABELLE project. While this $395M program would be substantial, its scope would be significantly reduced from that envisaged at the higher budget level.

A sustained $360M DOE/$32M NSF level would require termination of ISABELLE, make the initiation of a new project in the mid-1980's impossible, and lead to one or more other drastic reductions in the previously recommended programs. Many physicists would undoubtedly leave the field and this irretrievable loss of competent and highly-trained personnel would perhaps be the most damaging result. This would deal a staggering blow to the health of the U.S. High Energy Physics Program which would necessarily be headed for a serious decline.

Besides developing the above long range strategies, the Subpanel has also considered other planning issues, including non-accelerator elementary particle programs, international collaboration, and long-range planning mechanisms, all of which are discussed in the body of the Report.

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* Between a steady $(395, 34)M level and the higher level, which assumes an early approach to $(440, 35)M, there is a difference of opinion within the Subpanel as to where the line should be drawn for abandonment of the ISABELLE project. Indeed some members felt that inclusion of ISABELLE construction even at the higher $(440, 35)M level would jeopardize funding for the other elements of the national program. Others felt that the Project must be sustained even at the $(395, 34)M funding level. The majority was of the opinion that it should be included at the higher level and not at the lower.

SECTION III

SCIENTIFIC ISSUES

A. Status of the Field and General Approach

In the 1970's there was extraordinary progress in our understanding of the constituents of matter and their mutual interaction. This progress has occurred in at least four interconnected areas. First, experiments have established the existence of several new fundamental fermions: the charm quark, the bottom quark, and the tau lepton. Second, we now have the SU(2) X U(1) theory of the unification of the weak and electromagnetic interactions; this theory approaches quantum electrodynamics (QED) in its predictive power. Third, the concept of the quark-parton structure of hadronic matter has been extended enormously both by experiment and by phenomenological theory. Fourth, we now have a candidate fundamental theory for the strong interaction, quantum chromodynamics (QCD), which rivals QED in its beauty, economy of formulation, and potential range of validity. Together these have led to what is often called the "standard model" for elementary particle physics.*

Although some aspects of this theory need crucial experimental tests, it does provide a comprehensive reference to which alternative theories and models can be--and inevitably are--compared. The standard model also provides a way to classify the potential significance of experiments and of future facilities.

However, we must be ready for surprises. It is often said--and history bears witness to its correctness--that the reasons stated in advance to justify construction of a facility turn out not to be the most important ones. One may argue that the present situation with respect to QCD and SU(2) X U(1) is similar to the status of QED in the past; that is, they are becoming well-verified theories. Hence, one may argue that the area where surprises can occur is much smaller than it was in the past. Nevertheless, even within the standard picture, there is an additional force not clearly associated with gauge interactions. This is the so-called Higgs sector, a somewhat awkward feature of the theory. There can be every expectation of surprises occurring in that general domain of phenomena.

We, therefore, identify three distinct ways of viewing the future contributions of the new facilities. We may view them in the context of: (1) testing the standard model; (2) searching for and/or discovering phenomena which at present are reasonably well-defined but which either contradict or lie beyond the standard model; and (3) discovering genuine surprises, which by definition cannot be discussed in specific terms at present. The relative weight to be given these three categories is a subjective question. Based on the above discussion, we caution that the relative weight may not be the same as one would have given even 5 years ago. Beyond this cautionary statement, the question of how to choose the balance is one of scientific instinct.

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*We define the standard model to mean three, and only three, generations of quarks and leptons, SU(3) X SU(2) X U(1) gauge theories for strong, weak, and electromagnetic forces, one Higgs boson, and CP violation via complex parameters in the mass matrix. Neutrino mass and mixing, as well as proton decay, are left optional.

In the following Subsection, we discuss the ways future facilities might contribute to the field. We shall see that from the physics point of view, an optimal high energy program requires a diversity of approach, with fixed-target physics, electron-positron colliders, electron-proton colliders, proton-proton colliders, proton-antiproton colliders, and major non-accelerator experiments all serving as important contributors.

B. Contributions of New Facilities Within the Context of the Standard Model

There are two crucial tests facing the thus far successful SU(2) X U(1) gauge theory of weak and electromagnetic interactions. The first is the discovery of the carriers of the weak force, the intermediate bosons $W^{\pm}$ and Z^0, and the second is the precision measurement of $\sin^2\theta_W$, the main free parameter of the model. While one anticipates the Z^0 to be discovered at CERN's 270 x 270 GeV protron-antiproton collider in the near future, significantly higher luminosity and/or energy may be needed to identify the $W^{\pm}$. Incisive study of the Z^0 (as well as W) production dynamics also requires these higher luminosities and energies. This study is not only a probe of the weak force, but is also a sensitive probe of the strong interaction at short distances. There are many other properties of the particles such as mass, width, and decay modes into various final states, which are of vital theoretical interest. In the case of the Z^0, these detailed properties are most easily studied with electron-positron colliders (such as LEP at CERN, SLC at SLAC or CESR II at Cornell). Precision measurements of the Z^0 width yield a count of the number of neutrinos in nature. Measurements of partial widths, angular distributions, and polarization asymmetries sensitively test the structure of the weak coupling. Electron-proton colliders will also make powerful contributions, especially with regard to the mass and detailed couplings of $W^{\pm}$ to quarks and leptons, which are competitive with information obtainable from a hadron-hadron collider.

Study of W pair production is very important because it tests in a basic way the gauge aspect of the SU(2) X U(1) theory. With the full energy of LEP, this process can be studied; indeed this has been a principal motivation for its high design energy. This process may also be accessible for study at ISABELLE and TeV I, although in either facility the experiment is difficult.

The second important goal is the measurement of the basic parameter, $\sin^2\theta_W$, of the electroweak theory. Precise information on $\sin^2\theta_W$ is also relevant to grand unified models of electroweak and strong interactions. This measurement must be done at high and low energy scales in order to measure radiative corrections. At high energies, polarization asymmetries in Z^0 decay, as measured in electron-positron facilities such as SLC, are a sensitive probe. At low energies, a variety of facilities are of importance, especially lepton beams such as will exist at TeV II, or even polarized electron-electron scattering at the SLAC linac.

Parameters of the mass and mixing matrix which determine the pattern of quark weak decays also need measurement. Here again fixed target facilities such as TeV II and AGS, where subtle issues may be addressed, are essential. In the case of the heavy quarks, SPEAR remains an excellent place to study couplings of the c-quark, while CESR and DORIS are the facilities at present for the study of b-decays. Many heavy quarks are produced in the TeV II secondary beams, and in the long run these may become a very fruitful source of information as well. Semileptonic production of b-quarks--especially at an electron-proton collider--may be yet another source of information on weak couplings of the b-quark.

Investigation of the weak couplings of the t-quark depends on its mass. Electron-positron colliders are optimal, if there is adequate energy to produce t pairs. For masses up to 100 GeV, the proton-proton and proton-antiproton colliders may provide information. Currently proposed electron-proton colliders are effective if the t-quark mass is less than 50 GeV.

In the standard model, CP violation has a specified origin in the quark mixing matrix. Again, a variety of facilities may contribute, notably TeV II, AGS, and CESR.

Neutrinos may have masses and mixing parameters themselves. This is an open question within the standard model but arises naturally in some extensions. An enormous range of phenomena must be studied with great sensitivity to settle these issues. Large underground or underwater detectors, nuclear reactors, and meson factories offer a broad range of possible neutrino production and interference effects. Fixed-target accelerator programs facilitate experiments that probe other possible ranges of masses and different mixings.

There is also, within the standard model, the task of finding the three most prominent missing particles--the tau neutrino, the t-quark and the Higgs boson. Direct detection of the tau neutrino appears to require TeV II. With respect to the t-quark and Higgs boson, no one knows what masses these particles have. They could be anywhere from 10-30 GeV at the low energy end to hundreds of GeV at the upper energy end. For low masses (less than about 40 GeV) electron-positron colliders appear optimal for both Higgs boson and top quark. For higher masses (up to 100 GeV) the proton-proton and proton-antiproton colliders may be excellent tools for finding the top quark. For the highest Higgs boson masses, LEP or an even higher energy electron-positron collider may be needed.

Study of the strong interactions focuses on refining the tests of QCD, the present leading candidate for a fundamental theory. The qualitative confirmation observed most cleanly in electron-positron and lepton-nucleon collisions (and also supported by lepton-pair and high transverse momentum hadron production in hadron-hadron collisions) will be subjected to stringent tests when the energy ranges are dramatically extended by the future colliders. To the classical tests of electron-positron total cross section, gluon jet production, study of quarkonium properties, and scaling violations in lepton-nucleon collisions (available in the future to electron-proton colliders), one may add gluon jet production in electron-proton collisions, and possibly, in proton-proton and proton-antiproton collisions. Production of dileptons--especially those from Z^0 or $W^\pm$ decay--and of jets and direct photons of extremely high transverse momentum are excellent tools for probing QCD at the smallest distance scales. The secondary beams of TeV II will allow a detailed study of the internal quark structure of a great many hadrons, as revealed by lepton pair production. Further tests of QCD and hadron structure will certainly be devised as the theory is improved.

The properties of the strong force at larger distances--including the question of quark confinement--are a vital element as well. Quark searches should be carried out as each new energy frontier is opened. Also the sensitivity of non-accelerator quark searches in bulk matter should be improved. The nature of the strong force at long distances is certainly reflected in hadron spectroscopy, and the existence of predicted structures such as gluonium is best studied at lower energy accelerators such as SPEAR, AGS, or the SLAC linac. Meson spectroscopy at SLAC and AGS energies is far from complete, and such work should be important in comparing properties of light quarks to heavy quarks.

High energy soft collisions at TeV II and all colliders probe a unique combination of long and short distance physics. This involves the measurement of elastic, total, and various diffractive cross sections, multiplicity and correlation studies, and inclusive spectra measurements. This subject should be codified into a comprehensive phenomenology; an example is the Reggeon calculus. The phenomenology should eventually be comprehended in terms of a fundamental theory of the strong interaction such as QCD. While such studies are in a nascent state, one can expect growing interest in the subject over the coming years.

Another area of interest lies in the possible production of quark-gluon plasma in high-energy heavy-ion collisions. This subject is new and at present being actively pursued. The capability of ISABELLE eventually to store heavy ions makes this an attractive prospect for the longer-range future.

C. Contributions of the Facilities to Physics Beyond the Standard Model

Any discussion which goes beyond the standard model must be regarded in a more tentative way than the discussion in the previous Subsection. The topical theoretical concepts (extended gauge groups, technicolor, supersymmetry, compositeness) which imply excursions from--or modifications of--the standard model lack coherence and internal consistency at present. They are subject to relatively frequent revisions. A recurrent theme of these efforts is dissatisfaction with the conventional description of the forces associated with the Higgs sector. As we mentioned before, it is especially in this area that one can most confidently expect surprises. While the theoretical ideas are unreliable, they do provide prototypes for what might actually be present, are guideposts for experimental searches, and may be reasonable bases for comparing the possible contributions of future facilities.

The electroweak SU(2) X U(1) theory may not be correct. There may be no intermediate boson or there may be extra bosons, especially Z^0's. Proton-proton or proton-antiproton colliders such as ISABELLE and TeV I are excellent facilities for carrying out a search for such heavier Z^0's. The range of mass sensitivity runs, in the context of gauge theories, from present lower limits of 30-60 GeV to somewhere near 300 GeV for both TeV I and ISABELLE, with the high luminosity of ISABELLE a significant advantage. In the context of the original Fermi theory, the sensitivity extends to 900 GeV for the TeV I collider. Electron-proton collisions also provide a quite sensitive probe of heavier W's and Z's. Direct production of heavier Z^0's in electron-positron machines is limited to the total maximum energy, but the existence of such particles at up to nearly twice that mass could be inferred from interference effects.

Alternatives to the standard picture typically predict a proliferation of new particles. The list includes heavier quarks, heavier leptons, charged Higgs bosons, leptoquarks, technihadrons, axions, and supersymmetric partners of conventional particles. Most (but not all) of these can in principle be found in electron-positron colliders such as SLC or CESR II, provided the mass of such particles is less than 40 or 50 GeV (the LEP II project may eventually extend this number to 100 GeV). Some others, such as leptoquarks, may be better found in electron-proton colliders. Conjectured particles with masses in the 50-200 GeV range may be produced with proton-proton or proton-antiproton colliders. Production models for many of these particles--including some of mass 200-400 GeV--indicate that the cross-sections are high enough for them to be detected. Massive axial gluons, associated with spontaneous breakdown of a chiral SU(3) X SU(3) color group, might be detectable up to a mass scale of 400-600 GeV. Detection of any of these particles in the midst of a large background is difficult.

Some approaches to improving and extending the standard model make definite predictions of new phenomena at distances so short or energies so high that relevant accelerator experiments are inconceivable on any reasonable time or budget scale. However, these phenomena can be explored by other means.

The simplest grand unified models of electroweak and strong interactions predict the existence of proton decay and magnetic monopoles. These are each the subject of major non-accelerator experiments. (See Section VI).

It is possible that particles we today consider pointlike may be composite extended structures. This is at present a matter of serious theoretical consideration, extending to many of the particles we have discussed. The search for compositeness is a basic driving force toward higher energies. Most of the new facilities can be expected to set strong limits on the scale of compositeness or to directly observe the effects. The sensitivity level for most of these facilities is in the several hundred GeV range.

D. Intangibles

The kind of analysis made in the preceding Subsections cannot be blindly trusted. History teaches that the course of scientific progress is tortuous, with discoveries of great significance coming from unexpected directions. For example, according to the classifications we have given, the best machine for discovery of the upsilon would have been an electron-positron collider; nevertheless, it was found in proton-nucleon collisions. One could hardly have anticipated at the time of construction of the AGS that discovery of CP violation would be one of its greatest contributions.

While by definition, we cannot talk in detail about surprises, the general capabilities of a given facility for providing surprises may be considered--and these are of great importance. Each facility has its own, often unique, capabilities. An evident capability of fixed-target facilities is their high effective luminosity and concomitant diversity of approach. One measure of this is that, even from the point of view of exploratory "frontier" science (as opposed to programmatic studies), such facilities approach obsolescence only very slowly. The discovery of the J/psi at the AGS occurred 14 years after commissioning of the machine. At this time highly exploratory experiments are being performed at the oldest facilities, namely the SLAC linac and the AGS. One has no reason to expect less longevity of the TeV II program at Fermilab.

With regard to colliders, the oft-stated and well proven argument on behalf of electron-positron colliders is "anything charged (with mass less than the beam energy) will be produced." With the next generation of electron-positron colliders, this can be generalized to "anything, charged or neutral, with electroweak interaction, will be produced." Furthermore, backgrounds tend to be relatively small, so that detection problems are minimized.

With the advent of QCD, a similar argument applies to the high energy proton-proton and proton-antiproton colliders: "anything colored will be produced." There the background is higher but the center-of-mass energy is in practice an order of magnitude greater. Thus only the hadron colliders such as TeV I and ISABELLE will have access to the highest energy processes.

Electron-proton colliders combine the clean signal of a structureless electron probe with the high energy and rate associated with a proton projectile. Here, anything that mixes leptons and quarks or lets them interact will be detected. In all cases, it is self-evident that the exploratory capabilities of the colliders are very strong.

Sophisticated non-accelerator facilities for particle physics also offer opportunities for discoveries in unexpected directions. They can use cosmic rays as a source of particles at energies far above those accessible to accelerators. The small flux of these highest energy particles must be compensated by use of enormous detectors and new detection techniques. Such large systems can also look for processes far rarer than those detectable by normal techniques.

SECTION IV

SUMMARY OF FACILITIES IN HIGH ENERGY PHYSICS

A. The U.S. Program

The U.S. High Energy Physics Program largely involves the exploitation of particle accelerators and storage rings located at three national laboratories supported by the U.S. Department of Energy (Brookhaven National Laboratory, Fermi National Accelerator Laboratory, and Stanford Linear Accelerator Center) and the Cornell University Newman Laboratory of Nuclear Studies supported by the National Science Foundation. A much smaller but important component of the program involving elementary particle experiments which do not use accelerators or storage rings is described in Section VI. This Section summarizes the properties and physics activities associated with the various U.S. accelerator facilities operating, under construction, and proposed. In addition accelerator facilities in other countries are also briefly described.

1. Fixed-Target Program at BNL

The fixed-target experimental program at BNL is based on the 30 GeV proton synchrotron, the AGS, which provides up to 10^{13} protons per pulse every 1.2 seconds for neutrino physics and, with a one second flattop, every 2.5 seconds for electronic counter experiments. A development effort is under way to accelerate polarized protons, which would provide a unique capability.

The experimental program principally focuses on studies of weak interactions via neutrino reactions and K meson decays, hadronic spectroscopy with the multiparticle spectrometer including searches for gluon bound states, and hyper-nuclear physics.

The AGS is to become the injector for the large proton-proton storage ring ISABELLE presently under construction.

2. Fixed-Target Program at SLAC

The SLAC linac is the highest energy and highest intensity electron accelerator in the world. Its energy is being upgraded to about 50 GeV in connection with the Stanford Linear Collider (SLC) development. Its future experimental program focuses on further electron-proton and electron-nuclear scattering experiments with and without polarized electrons, beam dump experiments, studies of charmed particle properties and photoproduction with the hybrid bubble chamber facility, and studies of the spectroscopy of K* mesonic systems with the LASS spectrometer.

3. Fixed-Target Program at Fermilab (Tevatron II Project)

The physics program at Fermilab has up to the present time used beams produced by a 400 GeV synchrotron. The Energy Saver Project involves the construction of a ring of superconducting magnets in a position just below the present synchrotron to provide cost effective operation up to an energy of 500 GeV. The Energy Saver is scheduled to begin operation in late 1982 or early 1983. The superconducting magnet ring has been designed to reach energies up to 1000 GeV.

The Tevatron II project is a program to upgrade the beam lines and experimental halls to 1000 GeV operation. The time scale is such as to make some beam lines available in late 1983 and complete the upgrade by about the end of 1985. The cycle time of the new accelerator (1 minute) will be five times as long as that of the present machine and the intensity per cycle will be about the same ($>2 \times 10^{13}$ particle/pulse). However, the increased energy will provide time-averaged particle fluxes as large or larger than those in the energy region presently available with 400 GeV protons. Furthermore, useful particle fluxes will be available up to about 800 GeV, and the 20 second flattop will improve the duty cycle by about a factor of four.

These new facilities will permit a rich and diversified experimental program. Some of the ingredients of that program will undoubtedly be studies of neutrino interactions including detection of the ν_τ and searches for oscillations, studies of structure functions of hadrons, heavy quark spectroscopy, and tests of QCD through studies of jet phenomena.

4. SPEAR

The SPEAR electron-positron storage ring at SLAC operates with a center-of-mass energy between 2.5 and 8 GeV. Since the turn-on of the PEP storage ring, its operating time has been divided equally between high energy physics and synchrotron radiation research. The only detector presently projected for operation at SPEAR after the spring of 1982 will be the Mark III magnetic detector recently installed in the West Interaction Area. Systematic studies of states involving the c-quark, and states produced by radiative decay of the J/psi will provide a rich program not duplicated anywhere else in the world.

5. CESR

The Cornell electron-positron storage ring CESR has been operating since 1979, principally in the center-of-mass energy range 9 to 12 GeV, although higher energies are possible. Recent modification of the interaction region has produced a significant luminosity improvement. There are two interaction regions of which one is occupied by a large general purpose magnetic detector and the other by a non-magnetic detector which emphasizes high resolution electromagnetic calorimetry.

The CESR energy range is ideal for the study of the physics of the b-quark, and the major thrust of both experiments will continue to be the spectroscopy of the upsilon $b\bar{b}$ states and the mesons and baryons containing one b-quark. This program will undoubtedly require a number of years for its completion.

6. PEP

The PEP electron-positron storage ring which can provide center-of-mass energies between 8 and 30 GeV went into regular physics operation at SLAC in early 1981. During the summer of 1981, the interaction regions were modified to a mini-beta configuration and substantial luminosity improvements are expected. Five large detectors are either installed or in the process of installation in the five interaction regions available for physics research. These detectors embody a very considerable diversity in capability, and should produce a correspondingly diverse research program. The principal effort at PEP will involve precise and careful tests of both electroweak and QCD predictions, the detailed study of quark and gluon fragmentation, systematic studies of two-photon processes, and possibly c- and b-quark spectroscopy.

7. Tevatron I Project

The Tevatron I project (TeV I) at Fermilab is a proton-antiproton collider which will provide a luminosity of about 10^{30} cm^{-2} sec^{-1} at a center-of-mass energy of 2000 GeV. A more detailed technical description and assessment are given in Section V.

The start-up date for this facility is expected to be in 1985. A large and sophisticated general purpose detector is under construction for one of the available interaction regions and a modest experiment is contemplated for the other available interaction area. TeV I will have to share its operating time with the fixed target program (TeV II). It should provide a window into very high energy processes including production of $W^{\pm}$ and Z^0 intermediate bosons, studies of jets at high energy, and searches for surprises and new phenomena.

8. ISABELLE Project

ISABELLE is a proton-proton collider at Brookhaven designed for a maximum center-of-mass energy of 800 GeV and a luminosity of about 10^{33} cm^{-2} sec^{-1}. A technical description and assessment are given in Section V. Start up of ISABELLE, if one assumes a reasonable funding rate, would come around 1988. ISABELLE will have six interaction regions and will be a dedicated facility not sharing time with other programs. The physics areas addressed by ISABELLE include studies of W and Z production, dilepton and high transverse momentum jet production, searches for the t-quark and for a variety of other particles conjectured by current theoretical ideas, and generally searches for new phenomena.

The high luminosity and flexibility provided by many interaction regions should provide the opportunity for experiments of high sensitivity to very rare phenomena, provided that backgrounds can be successfully controlled.

9. SLAC Linear Collider (SLC) Project

The SLC project is a program at SLAC to achieve collisions of high-intensity micron-sized bunches of electrons and positrons with a center-of-mass energy of about 100 GeV. These bunch pairs arrive at the collision point at the pulse rate of the SLAC linear accelerator (180 per second) and after the collision the electrons and positrons are disposed of in a dump. The SLC project is a proposed construction project whose justification is twofold: (1) development of techniques for construction of linear electron-positron colliders whose energies might reach beyond those possible with circular colliders; and (2) an early look at the physics of the Z^0. A more detailed description and technical assessment are given in Section V.

B. The European Program

The European program in experimental High Energy Physics is centered at CERN in Geneva, Switzerland, and DESY in Hamburg, West Germany. CERN has 3,600 staff members, a yearly budget of approximately 600 MSF, and roughly 1,800 high energy physicists are involved in the program. The corresponding numbers for DESY are a staff of 1,100 and a yearly budget of approximately 130 MDM. Although DESY is a national laboratory, it is exploited internationally with groups from 11 nations and a total of 400 physicists participating in the program. A substantial European effort in non-accelerator based research is discussed in Section VI. At CERN, the center of activity is the SPS which provides 450 GeV protons for the fixed-target program and also serves as a 540 GeV proton-antiproton collider. A vigorous program is also carried out at the ISR utilizing both proton-proton and proton-antiproton collisions. CERN plans to phase out the ISR with 1983 as the last year of operation in order to free funds and personnel for the LEP project. Furthermore, CERN has decided to construct a low energy proton-antiproton facility, LEAR, based on technology developed for the high energy proton-antiproton project. For its future program CERN will construct a large electron-positron colliding ring, LEP. The first stage of this project aimed at providing electron-positron collisions at 100 GeV in the center-of-mass in four interaction regions has been approved by the CERN council. According to the CERN schedule, the first beams are expected toward the end of 1987 or the beginning of 1988.

The program at DESY is based on the electron-positron colliding rings DORIS and PETRA with the 7 GeV synchrotron used as an injector and to provide test beams. A proposal for HERA, a large electron-proton colliding beam facility with a center-of-mass energy of 314 GeV, has been prepared and will be submitted formally to the German Government shortly.

The following discusses the facilities mentioned above in more detail.

1. CERN

a. Fixed-target operation of the SPS

The SPS is the mainstay of the experimental program at CERN. The maximum energy has been upgraded to 450 GeV. The intensity with two batches injected from the PS is now about 2×10^{13} protons approximately every 10 seconds. The SPS has a very strong experimental program. The West Experimental Area will be upgraded to provide two high energy, high quality secondary beams in addition to several test beams.

New neutrino beams for beam dump experiments and for neutrino oscillation experiments are being constructed. The fixed-target program is expected to remain strong during LEP construction.

b. The proton-antiproton colliding project

The aim of this project is to produce proton-antiproton collisions at a center-of-mass energy of 540 GeV with a peak luminosity of 10^{30} cm^{-2} sec^{-1} in two intersection regions. This is achieved as follows: 26 GeV/c protons from the PS are used to produce 3.5 GeV/c antiprotons which are stored and stochastically cooled in an accumulator ring. After a sufficient number have been accumulated, the cooled antiprotons are reinjected into the PS, accelerated to 26 GeV, and transferred to the SPS.

The construction has been completed and the facility is now being commissioned with two of the five approved experiments taking data. High intensity bunches have been injected and stored with long lifetimes and tuneshifts of the order of 0.0025 have been observed. A luminosity of greater than 10^{27} cm^{-2} sec^{-1} has been obtained, and a luminosity of 10^{28} cm^{-2} sec^{-1} should be reached in the near future. The intention is to run proton-antiproton roughly 1/3 of the available time, but the precise time division between fixed target operation and proton-antiproton collisions will depend on the physics output.

c. The ISR

At present the ISR has a strong experimental program. The machine performs well with peak luminosities exceeding 10^{32} cm^{-2} sec^{-1} for proton-proton collisions in the low beta intersection region at a center-of-mass energy of 62 GeV. This has been achieved with the use of superconducting quadrupoles to focus the beam. A luminosity of 9×10^{26} cm^{-2} sec^{-1} has been reached in proton-antiproton collisions, and substantial amounts of data have been collected. The present intention is to turn off the ISR at the end of 1983.

d. Medium energy physics

The medium energy physics program research is fed by the PS. The emphasis is on antiproton and kaon physics at very low energies. As a spinoff of the proton-antiproton project, CERN is constructing a low energy antiproton facility, LEAR. Cooled antiprotons from the accumulating ring will be reinjected in batches into the PS, decelerated to 0.6 GeV/c and transferred to a small storage ring, LEAR. LEAR has different modes of operation. It can be run as a stretcher ring with 1000 s spill time delivering an external beam of 10^6 antiprotons per second with a momentum between 2.0 GeV/c and 0.1 GeV/c. It can also be run as a storage ring, either as a minicollider (proton-antiproton at 4.4 GeV) or as a source of antineutrons via the charge exchange reaction $p\bar{p} \rightarrow n\bar{n}$. Compared to present external beams, LEAR represents a large improvement in beam purity, intensity, and duty cycle. LEAR is expected to start operations in 1983.

e. LEP

LEP is an electron-positron storage ring with a circumference of 26 km and at most eight interaction regions. Phase 1 of LEP, aimed at providing 50 x 50 GeV electron-positron collisions in four intersection regions, was approved by the CERN council at its October meeting. The predicted peak luminosity is 10^{31} cm^{-2} sec^{-1}. The CERN schedule calls for the first beams by the end of 1987.

In Phase 1, only 1/6 of the available space along the ring will be equipped with conventional cavities. With the full complement of conventional cavities, LEP will be able to reach 2 x 85 GeV in the center-of-mass. However, if the superconducting technology is developed by the end of the 1980's, an energy upgrading of LEP will presumably be based on this technology. The center-of-mass energy could be 250 GeV with a gradient of 5 MV m^{-1}. A four-cell superconducting cavity has reached 2.5 MV m^{-1}.

It is premature to speculate on what the next step at CERN might be. However, it seems rather natural to install in the LEP tunnel a superconducting magnet ring suitable for storing 5 to 10 TeV protons. This would provide a rather impressive energy range for electron-proton or proton-antiproton collisions.

2. DESY

a. DORIS

The electron-positron collider ring DORIS is now being rebuilt with more RF added to raise the maximum center-of-mass energies to 12.0 GeV.

Both interaction regions will be equipped with mini-beta optics with an expected integrated luminosity of 300-400 nb^{-1} a day. DORIS will resume operation in May 1982 with two complementary detectors, a large solenoid detector ARGUS, and the Crystal Ball detector from SPEAR. A detailed study of bound $b\bar{b}$ states and of b-decays will be carried out.

b. PETRA

The electron-positron colliding ring PETRA has so far operated at center-of-mass energies between 10 GeV and 36.7 GeV. The installation of a mini-beta system in all four interaction regions has resulted in a peak luminosity well above 10^{31} cm^{-2} sec^{-1} and an average integrated luminosity between 400 and 600 nb^{-1} per day per interaction region.

A program has been initiated aimed at increasing the maximum center-of-mass energy to about 45 GeV in two steps. An increase from 36.7 GeV to 40.5 GeV will be achieved by mid-1982 with a doubling of the RF power from 4MW to 8MW. With the installation of RF cavities in the two remaining long straight sections the energy will be increased from 40.5 to about 45 GeV by early 1983.

Superconducting RF cavities are required to increase the energy beyond 45 GeV up to a maximum of 60-70 GeV. A single cell superconducting cavity built by Karlsruhe has successfully been tested with high RF power and will be installed at PETRA in 1982. DESY in collaboration with CERN and Wuppertal has a program to build and test multicell cavities.

c. HERA

DESY has proposed the construction of a large electron-proton colliding beam facility, HERA, designed to collide either electrons or positrons of 30 GeV nominal energy with protons of energies up to 820 GeV yielding 314 GeV in the center-of-mass and a maximum momentum-transfer-squared, Q^2 of 98,000 GeV^2. This is equivalent to an electron beam of 52 TeV on a stationary target. The design luminosity of around 10^{32} cm^{-2} sec^{-1} is sufficient to explore both charged and neutral current processes in the Q^2 region up to 40,000 GeV^2.

This machine has received strong support from the European High Energy Physics community and has been recommended for construction by a Government committee set up to evaluate the merits of HERA and other large scientific projects. This committee stressed the need for international collaboration both in the construction and the exploitation of HERA. The Senate of the City of Hamburg, which will contribute 10 percent to the total construction cost, has approved the project.

A detailed technical proposal for HERA has been prepared in collaboration with some 22 European institutions and will be formally submitted to the German Government shortly. The construction time of the project is estimated to be 7 years.

C. The Japanese Program

Elementary particle research in Japan has so far been carried out at the 12 GeV proton synchrotron at KEK. The future program involves the construction at KEK of a large storage ring complex, TRISTAN, designed to collide electrons and positrons and, at a later stage, electrons (or positrons) with protons. The TRISTAN ring will have a total circumference of 3016m and four interaction regions. An electron-positron colliding ring capable of exploring center-of-mass energies up to 60 GeV will be built. A maximum luminosity of 1.5×10^{31} cm^{-2} sec^{-1} is expected at 57 GeV. This project has been authorized and construction is now underway with the start of the experimental program scheduled for April 1986.

In a second stage a proton ring with 4.5 tesla superconducting magnets will be added. This will provide a proton energy of 300 GeV. Collisions of the proton beam with the 25 GeV electrons will yield a center-of-mass energy of 173 GeV, equivalent to 16 TeV for a fixed-target electron machine. If authorized by 1984, this project should be completed around 1990.

KEK, in collaboration with industry, has a strong program to develop superconducting cavities. High field superconducting magnets are developed in collaboration with BNL, Fermilab, and industry.

D. The USSR Program in High Energy Physics

Historically, research in high energy physics in the USSR has been carried out at five facilities: The Institute for High Energy Physics (IHEP), Serpukhov; the Joint Institute for Nuclear Research (JINR), Dubna; the Institute of Nuclear Physics (INP), Novosibirsk; the Institute for Theoretical and Experimental Physics (ITEP), Moscow; and the Institute of Physics (IP), Yerevan. The accelerator facilities at three of these (JINR, ITEP, and IP) are obsolete by modern standards, with center-of-mass energies of the available particle beams below 5 GeV, and are no longer considered major facilities; although Dubna does maintain an active program in high energy physics. The two major facilities now active for research in high energy physics in the Soviet Union are IHEP and INP.

1. IHEP (Serpukhov)

The major proton facility in the USSR is the 76 GeV synchrotron at IHEP. From 1967 until 1972, before Fermilab began operating, this accelerator was the highest energy proton source in the world. Although the proton beam intensity is low by Fermilab and CERN SPS standards, an active program of physics is being pursued. The research program is similar to the one at the AGS at BNL, with emphasis on large, relatively permanent experimental set-ups. There is a neutral beam in which some unique work on charm production by hadrons is being done and a neutrino counter experiment which is in limbo until the new booster comes into operation to increase the intensity. One group has been working on K^0 regeneration experiments for 10 years, and a large multiparticle spectrometer set up has been looking at charmed particles. There is a group doing emulsion work which has provided the cold emulsions for the neutrino search for charm in the 15-foot Fermilab bubble chamber. SKAT, the large propane chamber for neutrino physics, is still in operation as is MIRABELLE for hadron physics. The bubble chamber scanning and measuring facilities at both Dubna and Serpukhov are extensive and recently upgraded.

In the future the 76 GeV Synchrotron will become the injector for a multi-TeV accelerator/storage ring, UNK. Phase I of UNK, which was authorized for construction in 1981, includes a 400 GeV booster synchrotron using conventional magnet technology and a 400 to 3000 GeV (3 TeV) synchrotron using superconducting magnets whose design is similar to Fermilab's. Phase I will provide fixed target beams to 3000 GeV maximum incident energy (75 GeV center-of-mass) and proton-proton colliding beams of 400 x 3000 GeV (2190 GeV center-of-mass energy). Phase II, which is still in the planning stage, will provide 3000 x 3000 GeV proton-antiproton colliding beams and, with the addition of a second superconducting magnet ring, 3000 x 3000 GeV proton-proton collisions. UNK, if successfully completed, will provide a high energy physics research capability for the USSR in the 1990's far beyond any facility being planned elsewhere.

2. JINR (Dubna)

Although the accelerator facility at Dubna is not competitive by modern standards, the high energy physics program at this laboratory remains active. The group is allocated approximately 30 percent of the research time at Serpukhov and maintains a group of physicists on site to support this research. At Dubna, there is work on quark models of the nucleus using heavy ion beams. There is also some fine polarized jet target research which employs a very advanced and effective solid state detector array.

3. INP (Novosibirsk)

The Institute of Nuclear Physics (INP) at Novosibirsk is the center of lepton interaction physics in the Soviet Union. The research centers around the VEPP-4 electron-positron storage ring, which is designed for a maximum energy of 7 x 7 GeV (14 GeV, center-of-mass energy) and has reached 3.1 x 3.1 GeV so far. This device has been used to make precision measurements of particle masses in the J/psi range (4 GeV). An older storage ring, VEPP-3, is used as an accumulator/injector for VEPP-4 which allows the electrons and positrons to polarize before injection. The main detector facility employs a 400-ton magnet with the field perpendicular to the plane of the particle orbits. While the physics research program at INP is considered modest by western standards, the contributions of INP to the development of accelerator technology have been exceptional. INP is reported to be presently involved with design of an antiproton source for use with UNK. This effort stems from the pioneering work at INP on electron cooling of proton beams.

SECTION V

TECHNICAL EVALUATION

The Technical Subgroup of the Subpanel, P. Reardon, E. Knapp, W. Schnell, A. Sessler with consultants R. Lundy, J. Rees, and C. Taylor visited Brookhaven on October 8-9, 1981. A Subgroup of P. Reardon and A. Sessler with consultants R. Jameison and J. Rees, visited Fermilab on October 1-2, 1981. W. Schnell of the Technical Subgroup visited Cornell on October 12-13, 1981, and two more members of the Technical Subgroup, E. Knapp and P. Reardon with consultants A. van Steenbergen, E. Paterson, and P. Wilson visited Cornell on November 19-20, 1981. E. Knapp, P. Reardon, and A. Sessler of the Technical Subgroup with consultants P. Sargent and A. van Steenbergen visited SLAC on November 3-4, 1981, and W. Schnell of the Technical Subgroup visited SLAC on November 30 - December 1, 1981. Comments on these projects are presented in this Section.

A. Description of the ISABELLE Project

ISABELLE is a proton-proton collider designed for a maximum energy of 400 GeV x 400 GeV. Proton beams circulate in opposite directions in two separate rings each of 3,834 meters circumference. Superconducting dipole magnets with 5 tesla field are required to achieve this. Separate rings, crossing each other only where collisions are desired, permit the use of unbunched direct current beams. This eliminates a number of intensity-limiting effects and leads to a higher beam-beam limit than would be obtained with bunched beams. As a result, intense beam currents can be stored and very high luminosities can be reached.

For ISABELLE, beam currents of 8 amps per ring are stacked at 29 GeV energy by multiple transfers from the AGS. These beams are then rebunched (at the third revolution harmonic), accelerated to the top energy, and debunched again. Collisions are provided in six interaction areas, four of which will be initially equipped for experiments. The two beams cross each other at 11.2 mrad in low-beta insertions with beta of 7.5 m vertically and 42.9 m horizontally. They will provide 2×10^{32} cm^{-2} sec^{-1} nominal luminosity with as much as 60 m free-space for the experiments. Considerably higher luminosities, at the cost of reduced free-space, can be reached by lowering the crossing angle and the beta functions in the interacting region. Maximum luminosities of about 5×10^{33} cm^{-2} sec^{-1} appear to be achievable in this way.

B. Comments on the ISABELLE Project

1. Management

Since N. Samios was appointed BNL Deputy Director for High Energy Physics, there has been an improvement in the ISABELLE management structure. Several critical policy decisions relative to ISABELLE have been taken (adoption of one of the new cable magnets, the 1984 goal for test of first sextant, etc.) and a reorganization is underway which should improve internal communications and more sharply delineate lines of authority and responsibility. There is a recognized need for the appointment of a Project Director and a search (difficult under the present circumstances) is underway to find a senior individual to take over the Project and accomplish the in-depth planning and scheduling, including the integration of the R&D effort into the construction program.

A planning committee has been formed and all of the magnet activities have been integrated into a revamped magnet division. Reorganization of the cryogenic, RF, and vacuum systems effort is underway and the morale of the key individuals of the Project is reasonably high, now that an attractive magnet option has been chosen. Overall morale has, of course, been eroded by the uncertainties associated with continuation of the Project, particularly in the light of recent budget cuts.

2. Magnets

BNL has decided to set aside the previous magnet design (which featured "braid" with the preload supplied by differential thermal contraction of the iron yoke) and to adopt a new design incorporating Fermilab-style Rutherford cable preloaded by bolted-up split iron yokes. As it turned out, this provided an excellent magnet which should lead to a satisfactory set of magnets for ISABELLE. The first two, short prototype magnets (CM-1, CM-2) have demonstrated excellent maximum field capability, namely 5.85 tesla (at 3.7°K) with essentially no training, and indicate that the coil and yoke design provide the necessary mechanical properties for large aperture high field magnets. The design also addresses field quality, ramp-rate sensitivity, electrical insulation, problems of differential thermal motion and quench protection.

At the time of the Subpanel's visit there was every reason to expect that 15-foot long magnets will exhibit the same stability as the 5-foot models. However, since the causes of magnet instabilities (premature quenching, etc.) are not well understood, one could not be sure of this design until a long magnet had been operated under forced-flow cooling conditions. BNL put appropriate emphasis on the achievement of this goal and a first 15-foot magnet was indeed successfully tested. It reached a maximum field of 5.35 tesla at 4.5°K with no training observed and 5.85 tesla at 3.7°K. A second full length magnet gave the same near-perfect performance. The elimination of training in such accelerator magnets is a significant achievement of the BNL group.

A preliminary plan for a magnet development program now exists which features the testing of materials and production techniques over the next several months while a production capability of one or more magnets per week and a matching magnet testing facility are developed. This program will involve the construction of approximately 35 magnets as part of the "learning curve" and lead into the production of at least

54 dipoles and 23 quadrupoles which would be installed in the existing tunnel for a sextant test. The early part of the "learning curve" phase will explore the cost effectiveness of various split yoke and support tube designs, the properties and control of epoxy used to impregnate the fiberglass insulating tape, the performance of correction coil packages incorporated into full length dipoles and quadrupoles, the ability to protect magnets with internal diodes and quench-propagating heaters, and similar items, some of which are undoubtedly not yet foreseen. All of these questions must be answered as quickly as possible in order that production of identical units may commence which will allow a test of field reproducibility and magnet reliability.

The major goals of the magnet R&D program are outlined as follows:

a. Continue to produce long coils using three iron yokes. Test six magnets by April 1982. This will establish reproducibility of training behavior and develop coil fabrication techniques.

b. By April 1982 several additional 5-foot models will have been tested, using three short iron yokes, to establish details of the end winding design for proper field quality.

c. By June 1982, one full-length dipole per week will be produced. This will continue until approximately 35 pre-production magnets have been tested.

d. By April 1984, approximately 100 magnets will have been built with 54 dipoles and 23 quadrupoles assembled into a sextant ready for test with beam.

e. By late 1984, these magnets will be cooled down and operated in the ISABELLE pulsed mode to provide an integrated systems test.

The latter portions of the production effort will provide a basis for more accurate labor and material costs for the magnets for the remaining 11 sextants.

This is an ambitious R&D schedule. With adequate financial resources and proper management, it can probably be met by the BNL group. Completion of this R&D program will provide an adequate test of magnet production capability and enable BNL to proceed with full-scale magnet production.

3. Cryogenics and Transfer Line

The overall cryogenic system for ISABELLE seems sound. There appears to be enough refrigeration capability at BNL for magnet testing. Problems experienced with prototype transfer lines, supplied by industry, seem to be due to poor design and workmanship. These can be solved by proper attention to inspection at the factory by BNL.

4. Other Accelerator Systems

The ISABELLE lattice appears to be a sound design. The arcs offer reasonable flexibility for a superconducting system, namely, the possibility of a few integers' common tune variation in the two planes and of up to one or two integers vertical-to-horizontal tune difference. The matched insertions containing the collision points offer enough flexibility to cope with unforeseen beam dynamics situations. The possibility is foreseen to lower the collision-point beta-function and the crossing angle of the two beams (with some redesign and refitting) below the nominal design values in order to gain extra luminosity at a later stage of development or to reach design luminosity should there be difficulties in achieving the design beam current of 8 amps. This seems to present a valuable safety factor in the basic ISABELLE design.

The vacuum system for a storage ring with high intensity coasting proton beams presents difficult design problems because of the need for very low pressure, efficient clearing of ionization electrons, and a high degree of cleanliness of the chamber walls to prevent pressure runaway by ion-induced desorption. The basic features of the ISABELLE vacuum system are taken over from the proven CERN-ISR design (e.g., in-situ bake-out and glow-discharge cleaning prior to installation), but additional problems arise from the need to insert the vacuum system in a superconducting magnet. The design of the ISABELLE vacuum system has reached a high degree of maturity and has undergone extensive and successful testing in the form of detailed prototypes. Production could therefore start at any time with full confidence.

The remaining accelerator systems have not received the same attention. Examples are injection, stacking, acceleration, bunch-to-bunch feedback (required to fill the ISABELLE circumference with bunches from successive AGS cycles at achievable flat-top field tolerance), beam diagnostics, and beam dumping. For most of these systems only conceptual designs or preliminary models of critical components exist, and several basic design choices have yet to be made. The acceleration system (three 3rd-harmonic cavities per ring) is considered a problem because of the combination of the required high voltage and low permitted impedance presented to the beam. Model work on this problem and on the design of the stacking cavity is in progress. For most of the other systems, detailed design work appears to be postponed to a later time in the construction schedule.

There appear to be no insurmountable problems involved in any of these systems and it does seem possible, and indeed justified under the present circumstances, to defer much of this work to a later date. It would seem urgent, however, to prepare a detailed plan and time schedule covering all the outstanding design and prototype work, from basic decisions to detailed engineering.

The stated initial luminosity goal of 2 x 10^{32} cm^{-2} sec^{-1} with 8 amps stored in each beam appears to be realistic. For comparison, the ISR with an original goal of L = 4 x 10^{30} cm^{-2} sec^{-1} at circulating currents of 20 amps per beam has now reached L = 1.2 x 10^{32} cm^{-2} sec^{-1} with the help of a low beta insertion and stored currents up to 40 amps. Thus, later achievement by ISABELLE of luminosity up to 5 x 10^{33} cm^{-2} sec^{-1} through improvements seems reasonable.

5. ISABELLE Construction Cost and Schedule

The Subpanel notes that the total of construction cost, R&D cost, pre-operating cost, and equipment cost (including detectors) from FY 1983 onwards would be about $500M (FY 1982 dollars). The Subpanel was concerned about the adequacy of the contingency allocated for magnet production in the interim cost estimate and believes that an additional $10M-15M may be required.

6. Remarks on Accelerator Physics

The ISABELLE Project will advance accelerator technology in two major ways. First, it will provide experience in this country with high luminosity proton-proton colliding beams. This experience has been acquired by CERN over the last decade, but in the United States there is no such experience. The second major contribution is experience with building and operating a superconducting accelerator. Such experience is being acquired at Fermilab, but the ISABELLE Project extends this important expertise to a second laboratory and a wider group of people.

The ISABELLE Project will also advance accelerator technology by providing experience with a different, wide-aperture, higher-field magnet than the Energy Saver magnet. The testing of a magnet string is an appropriate and important intermediate milestone. It properly focuses attention on a major integrated systems test. Coupled with a coil production R&D program this magnet R&D is essential for any hadron accelerator construction project at Brookhaven utilizing superconducting magnets.

C. Description of the TeV I Project

The TeV I project is a proton-antiproton collider which will provide a luminosity of about 10^{30} cm^{-2} sec^{-1} at an energy of 1000 GeV x 1000 GeV. To achieve this goal, protons from one booster batch are to be accelerated, in 80 bunches, in the Fermilab main ring to 120 GeV. The proton bunches are then compacted by radiofrequency manipulation from 9 nsec to less than 1 nsec, and extracted and focused to a spot of 0.38 mm on a tungsten target. The antiprotons which are created are collected by a short focal length lithium lens and are then injected into a debuncher ring. This ring, which has a central momentum of 8.9 GeV/c, has an acceptance of ±2 percent in momentum and 20π x 10^{-6} m-rad in each transverse dimension and thus can accept 1.25 x 10^{8} antiprotons from each booster batch. The antiprotons are debunched by a factor of 10 in the debuncher ring and then extracted and injected into an accumulator ring. In this ring, by means of stochastic cooling, the antiproton density in both transverse dimensions is increased by a factor of eight, while the longitudinal (energy) density is increased by a factor of 10^4; so that finally 6×10^{11} antiprotons are in a core of 1.1 MeV. Approximately 6×10^{10} antiprotons, which are accumulated in 1.3 hours, are taken from the accumulator, and transferred to the main ring, and accelerated to 150 GeV and then transferred to the Energy Saver. This sequence is repeated three times. The resulting proton-antiproton luminosity, with a total of three bunches in each beam and a β^* value of one meter, is 10^{30} cm^{-2} sec^{-1}.

D. Comments on the TeV Project

1. Specific Remarks

The attainment by the TeV I project of luminosities of the order of 10^{30} cm^{-2} sec^{-1} usable for experiments depends on: a) the satisfactory performance of the Energy Saver ring as a storage ring; b) the success of the cooling systems in concentrating the antiproton beam to the necessary densities in phase space; and c) adequate antiproton production rates by the main ring and antiproton target system.

a) The Energy Saver ring will have a usable aperture of 15 mm over which the magnetic guide field is of sufficient quality for storage ring service. An orbit correction system is being built into the superconducting magnet ring which is designed to reduce residual orbit distortions to 1 millimeter or less. Given the expected transverse emittances at 150 GeV (the energy at which the beams are their largest) the transverse dimensions of the beams will be only a few millimeters at the worst place on the circumference. Taking orbit distortions and beam size together, only about 30 percent of the aperture need be occupied by 95 percent of the beam particles at 150 GeV and correspondingly less at 1000 GeV. These figures appear quite safe.

The designs of the low-beta insertions for the interaction regions are not yet complete. Designs for interaction regions beta-functions of 1.0 meters in both horizontal and vertical planes are in hand (at least conceptually), and these values yield $L = 0.5 \times 10^{30}$ cm^{-2} sec^{-1} with one bunch. (At the SPS an insertion with $\beta_x \leq 2$ meters and $\beta_y \leq 1$ meter is already in operation for proton-antiproton collisions.) In TeV I, luminosities up to 5×10^{30} cm^{-2} sec^{-1} should be attainable with multiple bunches in each beam.

The colliding beam strength parameter, ξ, (approximately equal to the beam-beam tune shift $\Delta\nu$), is only 0.002 to reach a luminosity of 10^{30} cm^{-2} sec^{-1}.

For the higher values of luminosity of 3 or 6 x 10^{30} cm^{-2} sec^{-1} the value is 0.003. For electrons where there are quantum fluctuations in the radiation and damping, there is considerable experimental information which shows successful operation of storage rings if ξ <0.02. For protons there is less knowledge. The ISR has an unbunched proton beam and in normal operation no beam-beam effect has been observed for ξ <0.001. TeV I has bunched beams, and hence an additional degree of freedom in the motion. First experience with proton-antiproton collisions at the SPS have shown that $\xi \leq 0.003$ is indeed an acceptable value.

b) In order to reach the proposed luminosities, the cooling ring is required to produce a cooled "core" of antiprotons in the stacked beam which is about 10^4 times denser in energy space (antiprotons per unit of energy spread) and eight times denser in each transverse phase space (horizontal and vertical) than in the injected batch. The central portion of the core must contain at least 10^{11} antiprotons per MeV within horizontal and vertical emittances of $2.5\pi \times 10^{-6}$ m-rad. (Emittances are defined to contain 95 percent of the distribution.) The concentration of the beam in the three dimensions of phase space is to be carried out exclusively by stochastic cooling systems similar, but not identical, to those used in the CERN Antiproton Accumulator now successfully in operation.

The rate of delivery of antiprotons to the cooling ring is anticipated to be about 10^8 particles every 2 seconds which leads to a collection time of 4 hours for 6 x 10^{11} antiprotons stored in the cooling ring and a central density of 10^{11} antiprotons per MeV in the core. Transfers of antiprotons to the Energy Saver for colliding beam operation will remove only 8 x 10^{10} particles for each batch transferred. At most, three such batches would be transferred to constitute a fill of the Energy Saver, and that loss could be replenished in about 1-3 hours. Four hours would be required to obtain the final goal of 5 x 10^{30} cm^{-2} sec^{-1}. If beam lifetimes of the order of a day are achieved in the Energy Saver, such collection times will not significantly degrade the average luminosity, but significantly shorter beam lifetime would threaten performance. This is an area of concern at CERN, but beam lifetimes of well over 10 hours have already been observed in the SPS proton-antiproton collider.

The CERN Antiproton Accumulator ring was designed to accumulate 6 x 10^{11} antiprotons in 25 hours. After one year of operation, it has achieved 1 x 10^{11} in about 30 hours. The TeV I design takes account of this CERN experience and overcomes the long accumulation time by means of:
(i) higher production rate by means of higher energy protons; and
(ii) enhanced capture via the lithium-lens, RF manipulations, and a buncher ring. Various longitudinal (radiofrequency) manipulations of the antiproton beam are required to introduce the particles into the cooling ring and to extract the central portion of the core from it. Although these procedures require precision, they are straightforward and do not appear to constitute a danger to the success of the project.

c) The antiproton production system uses a tungsten target and a lithium-lens collection system. The antiproton yields are calculated from a phenomenological fit to the world's cross-section data which shows the yield into the momentum acceptance of the cooling system to be rather flat over the range of central antiproton momenta from 8 GeV/c to 13 GeV/c. With an incident proton spot size of $\sigma = 0.38$ mm and energy of 120 GeV, the yield of 8 GeV antiprotons into the downstream acceptance ($A_x = A_y = 20\ \pi \times 10^{-6}$ m-rad and $\Delta p/p = \pm 2$ percent) is 5×10^{-5} antiprotons per proton. This spot size should permit targeting, without thermal destruction, the 3×10^{12} protons once every two seconds required to produce the 10^{8} antiprotons for the cooling system.

Fermilab already has a lithium-lens of smaller aperture while the collaborators at Novosibirsk have built another lens which would meet the specifications of the present application. There is no experience yet with a lithium-lens in a beam.

2. Gener [illegible]

The TeV I [illegible] certain risks associated with it such as beam manipulation by RF, stochastic cooling, beam-beam collisions with bunched non-radiating beams and efficient collection of antiprotons. On the basis of experience being acquired at CERN on the Antiproton Accumulator ring and the SPS collider, we believe that these risks are acceptable. Problems, if they arise, will often be outside the main ring enclosure and, hence, will delay colliding-beam physics, but not fixed-target physics. The attainment of a luminosity of 10^{30} cm^{-2} sec^{-1} would appear to be a reasonable goal. We note that this achievement requires efficient antiproton collection, effective cooling of the antiprotons, good manipulation and transfer of the antiprotons, and a sufficiently high beam-beam limit, but we believe that all of these requirements can be met.

The achievement of a luminosity greater than 10^{30} cm^{-2} sec^{-1} will, in our judgment, take some years of operational experience. On the other hand, a number of improvements seem possible. Thus, an ultimate goal of $L \cong 10^{31}$ cm^{-2} sec^{-1} appears reasonable to us.

A major reorganization has been accomplished by integrating the accelerator and Energy Saver effort with the TeV I effort. The Subpanel is impressed with the group and feels that it is basically quite adequate to be given the considerable responsibility of the TeV I project. However, some additional help may be required in the area of the accumulator and the damping ring.

The Subpanel notes that the total of construction cost, R&D cost, pre-operation cost, and equipment cost (including detectors) from FY 1983 onward would be about $70M (FY 1982 dollars). Detailed schedule and costs of the project are expected to be available by March 1982. The Subpanel believes that an additional $15M may be required to complete TeV I. With the annual funding constraints expected, the start up may be delayed until the end of 1985.

3. Remarks on Accelerator Physics

The TeV I project will advance accelerator technology in a number of ways. Most importantly, we shall gain experience in this country with stochastic cooling of antiprotons and with the operation of a high luminosity proton-antiproton storage ring. Experience with the operation of a superconducting ring is vitally important, but will presumably be gained with the Energy Saver project. In the future, stochastic cooling should become a readily usable tool of accelerator physics and, as such, will have many applications. Foreseeable applications are to a multi-TeV proton-antiproton project or, more immediately, to cooling in the Energy Saver ring itself. The latter could result in improved performance of the TeV I project. Thus, experience with an operational cooling system is a most valuable attribute to the TeV I project.

Future hadron colliders may have bunched beams and cooled antiproton beams, hence, the TeV I project including the associated detector developments will give experience which is applicable to future colliders.

E. Description of the Stanford Linear Collider (SLC) Project

The SLC, or Stanford Linear Collider, is a new type of colliding beam accelerator system which collides two bunches of particles accelerated in the SLAC linear accelerator at a single interaction point via a single collision, with the interaction rate made adequate by an extremely tight focus of the beams and very high charge in the individual beam bunches. The SLC provides electron-positron collisions at an ultimate energy of 50 GeV x 50 GeV and a design luminosity of $6 \times 10^{30}\ \mathrm{cm}^{-2}\ \mathrm{sec}^{-1}$, with a possible future increase in energy to 70 GeV x 70 GeV if that is required by the physics program. This project has a two-fold justification: the study of Z^0 physics at a center-of-mass energy of 100 GeV; and the development of a new accelerator concept which removes the energy limitations imposed on circular electron-positron colliders by synchrotron radiation.

The main components of the project are the energy upgrade of the SLAC linac; a transport system from the end of the linac to a small aperture magnet arc; the magnet arc itself; a special focusing system near the interaction point or two such systems for the alternate interaction points; the necessary housing; an experimental hall and staging area or two such areas; a high power positron-production target; a positron booster; a transport system from the positron target at the two-thirds point of the linac back to the injection end of the linac; a new high-peak-current electron gun; two small storage rings to reduce the emittances of the electron and positron beams by radiation damping; pulse compressors to reduce the length of the bunches in the storage ring before injection into the linac; and the necessary instrumentation and control systems for both the linac and the collider system.

A typical operation cycle is as follows. The electron and positron damping rings each contain two bunches of 5×10^{10} particles at an energy of 1.2 GeV. One of the positron bunches is extracted from the damping ring, passes through a pulse compressor which reduces the bunch length from the centimeter typical of the storage ring to the millimeter required for the linac, and is then injected into the linac. Both electron bunches are extracted from the electron damping ring, pass through an independent pulse compressor, and are injected into the linac behind the positron bunch. The typical spacing between bunches is about 15 meters in the linac.

The three bunches are then accelerated down the linac. At the two-thirds point, the trailing electron bunch is extracted from the linac with a pulsed magnet and is directed onto a positron-production target. The positron bunch and the leading electron bunch continue to the end of the linac, where they reach an energy of 50 GeV.

At the end of the linac, the two opposite-charge bunches are separated by a DC magnet, pass through a transport system which matches the focusing of the linac to that of the main collider ring and then begin to travel around the ring in opposite directions. The collider ring is composed of small-aperture magnets with very strong alternating-gradient focusing, which is required to hold down emittance growth in the collider arcs. After emerging from the arcs, the bunches pass through an achromatic matching and focusing section which focuses the beams to a very small size at the collision point.

The positrons produced by the "scavenger" electron bunch that was extracted at the two-thirds point of the linac pass through a focusing system at the positron source, a 200 MeV linear accelerator booster, a 180° bend and an evacuated transport pipe located in the existing linac tunnel. This brings the positron bunches back to the beginning of the linac. At this point, the positron bunch passes through another 180° bend, is boosted to an energy of 1.2 GeV in the first sector of the existing linac, and is then injected into the positron damping ring.

Because the emittance of the positron beam is very much larger than that required for collider operation, a positron bunch must remain in the damping ring for approximately four radiation damping times, which corresponds to twice the time interval between linac pulses. Thus, the positron bunch to be used in the next linac cycle is the one that is still stored in the damping ring from the previous cycle.

Electrons for collider operation are produced from a special gun equipped with a subharmonic buncher and located at the beginning of the linac. Two bunches of electrons are produced, are boosted to 200 MeV in a dedicated section of linac, and are then injected into the same section of linac used to boost the positron bunch to 1.2 GeV. At the end of this section, the 1.2 GeV electrons are injected into their own damping ring. The electron bunches at the time of injection into their damping ring have an emittance somewhat larger than required for collider operation but considerably smaller than the emittance of the positron bunch and thus need only be damped for two damping times or one interpulse period. The entire cycle repeats 180 times per second.

To achieve 50 GeV, a substantial energy upgrade is required on the existing SLAC linac in addition to the construction required to bring the two beam bunches into collision, as discussed above. Polarized electron beams are possible in the SLC.

F. Comments on the Stanford Linear Collider (SLC) Project

1. Introduction

The SLC represents a novel and exciting "window on the future" for high-energy colliding beam physics in the next decade, and by utilizing the existing SLAC linac, provides a mechanism for the U.S., to quickly achieve 100 GeV electron-positron collision energies for physics research at reasonable cost. As outlined below, the SLAC staff has addressed a host of technical questions associated with the SLC. Remarkable progress has been made in the understanding of the physics associated with such a machine, and, while the project is clearly not risk-free, it has a high probability of technical success.

Emphasis has been placed on the near-term problems associated with the conversion of the linac to collider operation. Comments follow on the individual subsystems and technical areas associated with the SLC project. The R&D associated with a very large linear collider aside from the SLC, such as very high-gradient accelerator systems or super power RF generators beyond an extension of present-day klystron technology, has not been initiated, and will not be discussed.

2. Cost and Schedule

SLAC has submitted proposals for the construction authorization of the SLC. The total cost of the project including two experimental stations (not operating simultaneously) with two sets of beam lines, detectors in both experimental halls (one of them being a major new one), a staging-area, linac improvements required for SLC, R&D, pre-operation to turn-on for physics, etc., is approximately $170M. The cost is about $30M less if existing detectors are used. If only one intersecting region is built, another approximately $20M will not be required. An additional $10-15M would be required if the klystron R&D were not successful and additional SLED II accelerating stations had to be added to achieve 50 GeV. The Subpanel recognizes that the construction of the beam transport tunnels and interaction region building are on the critical path for completion of the SLC in 1986.

3. Project Management

Because the project has not yet been officially approved or sanctioned, a project organization has not yet been established. The capabilities certainly exist within SLAC to accomplish the effort, and no major difficulties should be experienced in this regard. The part time efforts of a number of people have successfully carried the R&D and conceptual design work done so far but this will not be sufficient much longer if a FY 1986 turn-on date is to be realized.

4. Comments on the Damping Ring, 1 GeV Buncher Section, and Arc Structure

A great deal of effort has been devoted to the design of the electron and positron damping rings in order to achieve the smallest possible phase space magnitude after the short dwell time in these rings imposed by the basic 180 Hz SLC repetition rate. By proper optimization of the lattice parameters, a strong alternating gradient, small aperture, high-field structure has been arrived at which appears optimum for its intended purpose and has minimum damping time parameters. Both from the point of view of lattice structure and engineering design, the damping ring structures are not very far from state-of-the-art designs with certain original and innovative features. Magnet construction makes use of precision "stamped" laminations and infinite length busbar "coil" configurations. The vacuum system presents a problem because of the small aperture and the electron ring requires clearing of ions produced in the residual gas. The details of this clearing system remain to be designed and tested.

Because of the danger of phase space dilution by wake fields in the 1-50 GeV electron linac section, the electron beam bunch is reduced in length by a sequence of acceleration and nonisochronous magnets in the transfer section between damping ring and linear accelerator. In addition, for this transport section, special requirements exist so that beam polarization can be properly manipulated. This rather complex beam transport system has been fully designed and seems ready for construction.

For the 180^0 arc structures, the phase space damping associated with the RF acceleration and synchrotron radiation is absent and the optimization is guided mainly by the (undesirable) potential for beam emittance growth. This has been taken into account and a lattice structure allowing minimum emittance growth by stochastic photon emission has been designed.

Theoretical understanding exists of the phase space growth due to dynamic misalignment (typical rms limit 0.25 μm) of the arc and final focus transport elements and the actual amplitude and frequency spectrum of structure movement has been measured for the SLAC site. The results of this study indicate that uncorrectable position jitter (ν > 30 Hz) is well below tolerance level and where the jitter approaches tolerance levels, its frequency spectrum is below 30 Hz, making feedback corrections possible.

5. Comments on the SLC Positron Source

Efforts to upgrade a positron source to the required level of performance for the SLC project have already shown that a satisfactory final system can be built to yield the required positron bunch phase space density. Target materials have been tested up to the required primary electron beam density, conversion yields are known and the collection solenoid has been tested.

6. Comments on the Klystron R&D and Various Energy Options

Continuing research and development directed toward increasing the energy of the linear accelerator is carried on independently of the SLC program. That program does, however, set important intermediate goals both in time and in the required accelerator energy (50 GeV at the SLC interaction point).

The energy program currently being implemented will produce 32 GeV at the SLC interaction point. This represents the SLED I mode of operation with all present klystron stations equipped with the 36-MW klystron operating with 2.5μsec pulse width. To date about two-thirds of the stations are so equipped, and the complete conversion will require two to three more years at present rates of tube attrition and production.

SLED II requires 5μsec operation of the 36-MW klystron, and is the next step in accelerator energy upgrade regardless of the particular option chosen to reach the SLC energy specification. SLED II with the present number of klystron stations will produce 43 GeV at the interaction point. The R&D for this step has shown, however, that SLED II operation at 36 MW is marginal and that any increased energy per station beyond SLED II will almost certainly require significant changes in tube design. These changes lie in both gun and output cavity areas, with improved window performance a likely requisite also. Experimental tubes incorporating such change are being built, and two of the four options under consideration for SLC at 50 GeV involve a new klystron which is a direct descendent of the present tube along these lines. One option considers an improvement to 49 MW using the present SLED cavities; the other to only 42 MW, but replaces the SLED cavity with a more efficient design.

A third option implies the successful completion of a joint SLAC-Japanese collaboration in the development of an entirely new klystron to operate to 150 MW with a 1 microsecond pulse. This R&D program is only about six months old, and is expected to continue for about two more years. It is too early for prognosis, but if successful, this approach should be favored both by virtue of simplicity (no SLED is required) and its implications for the future.

The fourth option does not require an increased energy gain per klystron, but simply increases the number of klystron stations (operating SLED II at 36 MW) by about 30 percent. This option requires no R&D, but is the most expensive, both with respect to capital and operating cost. It is this option carried to an extreme (doubling the number of stations) and in combination with one of the other three which could increase the SLC interaction point energy to 70 GeV should that be desirable from a physics point of view in the future.

Current klystron production is sufficient for complete turnover of klystron complement in about five years. This production is limited by capital equipment rather than by personnel skills, and could be at least doubled while retaining good cost effectiveness.

7. Comments on the Beam-Beam Interaction

SLAC has studied the effect of one beam on the other extensively. The interaction will produce significant changes in beam size and scattering. Thus, the phenomenon is important in estimating luminosity and in estimating the orbits of electrons downstream of the collision point (which is important for estimating experimental backgrounds and for designing the beam dumps).

The interaction of one beam on another is measured by a dimensionless parameter, called the disruption parameter, D, which is given approximately by the ratio of bunch length to the focal length produced by the beam-beam focusing. For D = 0.8, the value expected for SLC, the luminosity is enhanced, due to beam-beam focusing, by a factor of 3 and this factor is included in the estimated luminosity of 6.0×10^{30} cm^{-2} sec^{-1}.

For D=1, the number of plasma oscillations is much less than one (~1/3) and the calculational techniques are reliable. Only for D~30 will instabilities become important and the numerical approach need modification. The Technical Subgroup thus felt that the beam-beam effect is well-understood, as a result of the SLAC studies, and provides an adequate basis for SLC design. In particular, the luminosity enhancement, due to beam-beam effects, appears to have been evaluated reliably.

8. Comments on the Final Focus

The final focus is required to match to the beams in the arcs, and transform these beams to exceedingly small transverse cross-sections at the point of crossing. The input parameters are taken to be 5×10^{10} particles in an emittance of 3×10^{-10} rad meters (at 50 GeV), and a momentum spread of ±0.5%.

At the present time, SLAC has three possible designs for the final focus section. One, the "microquad" solution, employs permanent (rare-earth) quadrupoles with a bore of only 1.0 mm at a distance of only 20 cm from the collision point. A second solution, the "miniquad" design also uses rare earth permanent magnets but with a bore of 1 cm. A third solution employs superconducting quads at 4.5m from the collision point and as in the "miniquad" design, chromatic aberration is corrected. The final choice has not yet been made, but the Subpanel feels that the existence of a solution has been adequately demonstrated.

9. Comments on One-Sector Results

Experiments are underway on Sector I of the SLAC linac to:

a. Develop a new high-intensity single-bunch injector;

b. Convert the present control system of the linac into a prototype for the SLC; and

c. Demonstrate that the single-bunch beam can be accelerated and injected into the electron damping ring, and generally that it can be accelerated without undue emittance growth due to wake fields.

So far, the new high-intensity, single-bunch injector has produced the required charge in a single-bunch (> 5 x 10^{10} electrons) but this bunch has not been successfully accelerated through Sector I. This is believed to be due to instrumentation difficulties. Sector I has been used to measure transverse wake field effects at lower currents by running the beam off axis in the accelerating section and observing beam spot distortion. These measurements indicate approximate agreement with the wake field theory. The emittance growth generated by transverse wake fields was considered one of the major technical unknowns in the project, and the successful measurement of this effect gives confidence that it can be successfully controlled. Sector I is expected to inject single bunches of 5 x 10^{10} electrons into the damping ring by the end of 1982 (when construction of the first ring will be complete).

10. Comments on the Buncher-Injector System

The R&D effort associated with the buncher-injector addresses the problem of producing a single beam bunch with the required charge and phase space specifications at the injector output (the 40 MeV point), and continues the development of high current polarized electron sources.

The buncher consists of four (two subharmonic and two fundamental) bunching sections, and has been demonstrated to capture about 80 percent of the charge emitted by a thermionic cathode pulsed for 1.5 nsec into a single S band bunch.

This part of the R&D has been successfully completed. The measurement of the output emittance represented only an upper limit and that limit was twice the SLC specification. The single bunch charge exceeded the SLC specification of 5×10^{10} electrons.

The polarized source R&D is unique in this country and we recommend its support independent of its applications to SLC. Peak current several times that required for SLC has been demonstrated. Improved cathode life and higher polarization are the goals of this program. With the nominal value of $\Delta E/E$ of ± 0.5 percent a depolarization by no more than 20 percent occurs in the arcs. On the other hand, any appreciable increase of $\Delta E/E$ will lead to a significant decrease of polarization.

11. Comments on the SLC R&D Program

SLAC has already made notable progress in their SLC R&D program. The development of a high-intensity gun and the measurement of linac jitter have gone a long way towards reducing the uncertainty--and hence risk--associated with the SLC project.

The Sector I tests show that the wake functions are not grossly different from those expected on the basis of theoretical calculations. Further studies are going on at present and will, presumably, soon give much more accurate results. On the other hand, efforts to transport a high-intensity bunch (10^{11} electrons) through Sector I were not successful. It is believed that this lack of success (i.e., emittance blow-up) is due to "trivial reasons." Further tests, with high-current bunches, are being done at present and will, hopefully, be successful. Nevertheless, the difficulty already experienced serves to show that SLC requires a very good understanding, and control, of linac performance.

It should be noted that the SLC will probably not fail--if it fails at all--in a "hard way." Rather, failure could occur partially, that is, by not achieving the design luminosity. There may be jitter in kickers so that, finally, bunches will not collide head-on, but miss somewhat; or if low-emittance bunches are not achieved then the luminosity will, again, be reduced; or if the amount of charge per bunch is limited by some unknown effect then, again, the luminosity will be affected.

The basic tests of electron bunch production, damping, and reinjection into the linac are scheduled to begin in early calendar 1983. These tests will be a very valuable indication of the future performance of the SLC.

12. Comments on the Performance Improvement and Contingency

There are a number of improvements in SLC performance which can, at least be contemplated. At this stage it is, of course, premature to seriously consider these improvements, but they do serve to show possible future directions for SLC.

Even a small increase in the number of particles per bunch, from 5×10^{10} to 6×10^{10} will increase the luminosity by a factor of 1.7. If six bunches were accelerated in the linac (rather than three) the luminosity would be increased by a factor of 2. If the crossing beta-value were reduced by a factor of 2, then the luminosity would be increased by a factor of 4. Thus, one can imagine the performance of SLC increased beyond the design luminosity of 6×10^{30} by as much as an order of magnitude at a later stage.

In case of difficulties a number of actions can be taken. For example, one can exchange bunch length with bunch transverse size and this freedom could be very useful in fighting linac tolerance. Damping rings are relatively inexpensive and one can contemplate building another one if necessary. One can also contemplate running the frequency of collisions down to 90 Hz (rather than the 180 Hz of the design). This will reduce the luminosity by a factor of 2, but it will reduce the average power requirement on klystrons, reduce the positron target requirement, while increasing damping times. Also, one can contemplate reducing the bunch size (say from 5×10^{10} electrons to 3×10^{10} electrons). The luminosity would be reduced by a factor of 4 and there would also be a reduction in the positron production requirement.

In short, there are a number of possibilities which make the gradual attainment of the design luminosity of SLC rather likely.

13. Remarks on Accelerator Physics

The SLC project will introduce several new technologies which may become part of the basis for future linear colliders of much higher energy. In essence these technologies are the acceleration of very dense bunches of electrons and positrons in linear accelerators, micro-beam focussing and beam-transport systems and the mastery of single-passage collisions.

G. Description of the CESR II Proposal

CESR II is a single-ring electron-positron collider of 5.49 km circumference, to be built at a specific site near Cornell University in Ithaca, New York. The nominal maximum energy is 50 GeV per beam, i.e., 100 GeV in the center-of-mass. The design is made for a luminosity 3×10^{31} cm^{-2} sec^{-1} at that energy. The beam optics designed to achieve this luminosity in each of the four interaction areas leaves 4 m free space for experiments in each area. The basic idea of the CESR II proposal is to provide the maximum possible luminosity at the anticipated Z^0 energy in a reasonable number of experimental areas and to do so at minimum cost. The possibility of achieving longitudinal polarization of the colliding beams without great difficulty or risk is another asset.

Large areas of the overall design are still in provisional or conceptual form only. To the extent that it is known, the design is that of a conventional electron-positron circular collider with one basically new feature: the chosen circumference is relatively small and hence the very large RF voltage of 1.5 GeV per turn is required to reach 50 GeV. To generate this voltage at reasonable power consumption and length of RF structure, superconducting cavities are compulsory. It is proposed to use a particular type of superconducting RF structure called the "muffin tin" developed at Cornell. The structure is to operate at the unusually high frequency for storage ring service of 1.5 GHz, and its total active length, made up of 1000 individual five-cell modules, is to be 500 m.

These superconducting cavities are at present still in the stage of laboratory models, not having reached nominal performance yet in any respect. Hence, most of the present effort by the Cornell group is R&D work concerned with the development of a suitable "muffin tin" structure, its testing with beam in CESR I, and preparations for an engineering design suitable for mass production. A second branch of the work is concerned with the consequences for beam stability and with other technical problems presented by the short and dense particle bunches which result from the choice of an unusually high RF frequency.

H. Comments on the CESR II Proposal

1. General Comments on Proposal-Status, Time-Scale, and Cost

Unlike the other three projects reviewed (ISABELLE, Tevatron I, and SLC), there is not yet a firm proposal for the construction of CESR II and the main emphasis of the activity is the development and testing of superconducting RF cavities. The laboratory made it clear that they would not submit a firm proposal for construction until they have satisfactorily demonstrated both that the superconducting RF cavities work and that mass production techniques for their fabrication have been established. They expect to be in a position in FY 1983 or FY 1984 to submit a proposal for an FY 1985 construction start. Adequate R&D funding for the superconducting cavity development is required ($2M in FY 1982, $4M in FY 1983, and $10M in FY 1984) in order for an FY 1985 start date to be realistic. This funding should be sufficient to demonstrate the feasibility and reproducibility of the present "muffin tin" superconducting cavity approach but is not sufficient for development of an alternative superconducting RF cavity approach. The Subpanel supports the Cornell position that additional funding for at least one such additional approach would be very desirable (for reasons explained below) to make the project ready for construction at minimal risk at the time proposed.

Although the Subpanel did not make an in-depth review of the conceptual design cost and schedule for CESR II, a 3-year construction period is considered to be optimistic. The total cost of the effort including construction, R&D, preoperating, and detectors is estimated by Cornell (on a very preliminary basis) to be about $340M (FY 1982 dollars). It is difficult to assess the adequacy of this estimate in the light of the present status of the superconducting cavity R&D. The laboratory plans to submit a more detailed cost and schedule estimate once it decides to go forward with a project proposal. Although there was some general discussion of the issue of staff required and the associated staff build-up necessary to accomplish a major project such as this, there was no discussion of project management as both the laboratory and the Subpanel thought this would be premature.

2. Expectations on Achievable Luminosity

The present overall design assumptions in CESR II, regarding two-beam behavior or beam-beam limits, are realistic and consistent with today's understanding and experience with existing electron-positron storage rings. Every attempt has been made to achieve as high a specific luminosity (luminosity per mA of beam current) as possible consistent with known limitations. Keeping the interaction regions short is an example of a lesson learned from the present generation of storage rings. However, the consequences of operating with a high betatron tune to achieve a small emittance is still under study. In all probability, this study will lead to some compromise between higher costs and attainable luminosity. Much work remains to be done on the lattice before a cost optimized design can be realized.

The area of greatest uncertainty in achieving high luminosity is in single beam limitations. The studies of transverse and longitudinal interactions between the beam and the 5000 cells of high-Q superconducting cavities, already show that to achieve longitudinal stability, the injection energy will have to be quite high. The beam behavior reacting to the transverse impedance of this structure is even more difficult to estimate. Extrapolating from instability limits in existing machines is difficult due to the very short bunch length in CESR II and the different character of the RF structure. Existing theoretical models indicate that there is a concern as to whether the required single bunch current can be achieved. This is an area which requires more theoretical and experimental work, as the leverage on achievable luminosity is large.

3. CESR I Beam Cavity Tests

A critical part of the superconducting cavity R&D program leading up to a final CESR II proposal is an active cavity test in the existing CESR I storage ring. This test represents the first experimental use of superconducting cavities in a storage ring configuration. Goals of the tests include measurements of:

a) Effects on the beam of beam-induced modes in the cavities.
b) Effects on the cavity of beam-induced modes.
c) Effects due to high synchrotron oscillation frequency.
d) Higher order mode power generation and a comparison with bench measurements of coupling impedance.
e) Cavity survival in an accelerator environment.
f) Beam lifetime versus voltage.
g) Beam capture at 1500 MHz.
h) Coupling of beam power through superconducting cavity systems.

These tests represent a critical examination of the practical problems associated with the use of superconducting cavities in storage rings and are extremely important for both a future CESR II proposal and the possible extension of technology to the next generation circular accelerator systems for high energy physics. They are receiving highest priority at Cornell, and should continue to receive this high priority whether or not the CESR II proposal goes ahead. Answers to the above listed questions are critical to determining the level of R&D support desired for superconducting cavity investigations in the future. The program seems well conceived and is being carried out in a skillful manner.

4. Comments on the Cornell R&D Program on Superconducting Cavities

The development of the CESR II superconducting accelerator structure is solely based on "muffin tin" structure made of niobium sheet and operating at 1.5 GHz.

The Cornell group has pursued the development of the "muffin tin" configuration over several years, and has progressed to the point where the mode spectrum of the cavity system is well understood and the fabrication techniques are adequate to produce cavities which have interesting Q and accelerating gradients. Grooving of the cavity surfaces seems to control multipactoring, a cause of cavity breakdown, which had limited cavity performance in the past. Study of this family of cavities through the active beam tests on CESR I is strongly encouraged, even though several new ideas have emerged during the last years which might change the final choice of system for CESR II after an adequate R&D progam. The cavity fabrication development program can produce one five-cell "muffin tin" type cavity per month in the near term, which will give a reasonable number of cavities for the CESR I tests and develop fabrication techniques at a reasonable rate. It is projected that two cavities per month can be produced in the future.

The "muffin tin" does, however, have the following disadvantages:

a) grooves covering the top and bottom surfaces are necessary to suppress multipactoring. This seems to work but the grooves increase the exposed surface and add to the manufacturing cost. By contrast, the axially-symmetric shapes now developed elsewhere are inherently safe from multipactoring as shown by computer simulation and by experiments at CERN.

b) alternate structures must be tilted by ±45° to eliminate synchro-betatron coupling and other transverse effects.

c) "muffin-tins" are not accessible to computations of fields and impedances. A circular symmetric "equivalent" is used for computations of higher modes and instabilities.

Therefore, work on a Geneva-CERN type axially symmetric cavity system should proceed to the extent possible in parallel with the "muffin tin" cavity program. The work just being started on using super-pure niobium for cavity fabrication, to achieve better heat conductivity and thus thermal stability is strongly encouraged.

The chosen frequency of 1.5 GHz is about the maximum possible for stable beam dynamics (high synchrotron tune, and short bunches). Technically, the high frequency imposes the costly 1.9°K technology and it leads to a very large number of inconveniently short modules (1000 five-cell modules, each only 50 cm long with two tuners, three waveguide ports and wasted space along the beam.) The Cornell group conceded much of this, stressing, rightly in the Subpanel's opinion, that the Cornell work is complementary to the CERN work with axially symmetric 500 MHz structures and that a final choice has yet to be made.

5. General Comments on R&D work at Cornell

The work done at Cornell in general and the R&D work on superconducting cavities in particular is of outstanding quality and represents a first rate effort in accelerator development. At present, most of this effort is directed towards CESR II and even in this specific context the field covered has been somewhat narrowed by the desire to make a specific design ready for construction in the shortest possible time, given the severe limitations of funding and manpower. The group is, however very well equipped--intellectually as well as materially--to carry out accelerator development of a wider scope, be it in the field of superconducting RF cavities for circular and linear accelerators in general or even outside that field. The Subpanel considers it of great importance that full use is being made of the outstanding potential of the Cornell Group, independent of a decision on going ahead with CESR II.

SECTION VI

NON-ACCELERATOR PHYSICS

A recent trend in particle physics is that some of the important problems are now being addressed by experiments which do not involve high energy accelerators. While experiments to search for proton decay represent the largest of these efforts, there are also such experiments, both proposed and operational, to search for quarks, monopoles, and neutrino oscillations and to investigate extensive air showers and cosmic ray neutrino interactions. We expect that the trend toward non-accelerator investigations will continue in the future.

Since some of the future non-accelerator projects are likely to be quite large and expensive, the current fraction of the high energy physics budget (1 percent) devoted to this work may need to be increased. For example, the possible establishment of a national underground laboratory for studies of proton decay and related experiments is being seriously discussed. Proposals to deploy a large neutrino facility deep underwater and to construct a large surface array for the study of primary cosmic rays are also under consideration. The construction and operation of such facilities would require significant resources, and in addition, may require an organizational structure other than the individual university approach used up to now.

We give here a brief description of current non-accelerator programs and then make a specific recommendation.

A. Neutrino Mass Measurements and Neutrino Oscillation Searches

Many experiments that focus on the possible non-zero mass of the neutrino are planned at facilities other than the major high energy accelerators. Reactors provide antineutrino beams with which one can test for oscillations via the disappearance of the $\bar{\nu}_e$'s. Medium-energy facilities, such as LAMPF, also provide low-energy ν_μ, $\bar{\nu}_\mu$, and ν_e's. Several experiments are planned or mounted which will study these neutrinos. Beta-decay studies, as in tritium, are being performed by several groups to improve present limits on the $\bar{\nu}_e$ mass and to check the recent results indicating a possible non-zero value of this mass.

B. Quark and Monopole Searches

The search for fractional charges on matter is of great consequence for understanding whether quarks can exist as isolated objects. One such experiment has reported positive results, other experiments report negative results, and there are many continuing experiments.

The production of magnetic monopoles in the early universe is expected on the basis of grand unified theories of weak, electromagnetic, and strong interactions. Designs for underground searches covering up to thousands of square meters are currently underway.

C. Cosmic Ray Physics

With the advent of large and sophisticated detectors and electronics, the time may have come to turn these techniques--so effectively applied at accelerators--to non-accelerator experiments as well. The physics of cosmic ray interactions at laboratory energies above 1000 TeV will not be studied by any other means in the near future.

The exploration of particle interactions at these ultra-high energies is currently being pursued at a number of laboratories using a variety of techniques. One example is the "Fly's Eye" detector now being brought into operation. The technique shows great promise and an extension of the present detector (costing up to approximately $20M) has been proposed. Further, a deep underground muon detector in conjunction with such an array has been proposed.

D. Deep Underwater Muon and Neutrino Detector (DUMAND)

The DUMAND (Deep Underwater Muon and Neutrino Detector) project has been proposed for studying very high energy neutrino interactions (> 1 TeV) and for astrophysics studies, many of which have scientific interest whose overlap with high energy physics is growing. The prototype detector consists of an array of about 1000 phototubes deep underwater. The initial array would cost approximately $5M and take about three years to build. At present, a five phototube muon "string" is being deployed to refine the technology and to measure high energy muon fluxes. The DUMAND detector is an example of a proposed large non-accelerator endeavor which would require substantial resources and an appropriate user-oriented structure that is not at present a part of the national laboratory system.

E. Proton Decay Experiments

Among the non-accelerator experiments, the confluence of physics potential and technological feasibility is most compelling for proton decay searches. Results from the first round of dedicated experiments should be available within the next two years.

If present experiments do not reveal positive evidence for nucleon decay at the 10^{32} year lifetime level, and if the backgrounds prove to be tractable, a detector at least ten times the size of those now being used, optimized to search at the 10^{34} year level, may be deemed worth its large cost. This is estimated at approximately $40M.

If the nucleon lifetime is established at a value near the present 10^{31} year limit, a more sophisticated class of experiments would be of paramount importance. These would provide our only window to the grand unification scale of 10^{15} GeV through study of the detailed modes of nucleon decay. The ideal detector would include the capability of particle tracking, particle identification, and momentum measurement. Fine grained configurations and magnetic fields probably would be necessary. Useful measurements of branching ratios would necessitate the observation of hundreds or thousands of events. We estimate the cost of such an ultimate (> 10 kT) detector to be about $50M. It is important to encourage R&D efforts leading to an appropriate design and depth for this detector.

Despite the current preeminence of the U.S. in this field, the initiative for a large, user-oriented facility has been taken in Europe. The excavation of a cavity at a depth of 5 kmwe (kilometers water equivalent) to house a fine-grained detector with a fiducial mass of about 10 kT is already underway at the Gran Sasso laboratory near Rome. Moreover, other European groups already are proceeding with the construction of a 1.5 kT fine-grained detector at the same depth, and a Japanese group is proceeding with the construction of a 3 kT detector at greater depth than the new U.S. detectors.

F. Recommendation

We endorse the present allocation of about 1 percent of the resources of high energy physics to non-accelerator experiments. It may be desirable to increase this proportion to accommodate new facilities, and we advocate a flexible response of the funding agencies to those initiatives that are outside of the accelerator framework.

SECTION VII

FACILITY RECOMMENDATIONS AND LONG RANGE PLANS

A. Overview

The future of high energy physics promises to be as exciting and productive as its profoundly successful recent past. Whereas the U.S. program has dominated this field until recently, the importance of high energy physics has stimulated vigorous competition from other regions of the world. This Subpanel concludes that the U.S. High Energy Physics Program has the facilities that can meet the challenges of the science as well as foreign competition well into this decade, that it lays the foundations for future development beyond this decade, but that it will require the construction of a new facility available for research by the end of the decade in order for the U.S. Program to continue to be vital into the 1990's. In current plans, ISABELLE is envisaged as this facility. As discussed in Subsection C, fiscal considerations may require that a different project be constructed. The Subpanel examined a number of possible projects, but considers it premature to recommend now a specific choice.

The older facilities in the U.S. Program, including the BNL fixed-target program, the SLAC fixed-target program, SPEAR, and the 400 GeV program at Fermilab, when supported adequately, remain productive and, as history shows repeatedly, are likely to uncover major surprises in physics. Nevertheless, their roles will inevitably decline over this decade as the field advances.

CESR and PEP have already made important contributions to electron-positron physics and are expected to continue to do so over the next several years. We can expect new kinds of detectors and experiments at these facilities, but their role may be declining beyond the middle of the decade.

Tevatron II will provide a large variety of high quality beams for fixed-target experimentation beginning in the early 1980's. Slightly later, the TeV I project will have two experimental areas for studying proton-antiproton collisions at 2 TeV in the center-of-mass. The Tevatron projects will be the focus of a major part of the U.S. Program during the middle of this decade. As discussed elsewhere in this report, they will open up entirely new areas of physics and accelerator development and will be essentially unique in the world.

The ISABELLE project (as discussed extensively in the Interim Report and elsewhere in this report) is designed to provide a major base for high intensity hadron experimentation at the end of this decade and into the next.

Finally, the SLC project is expected to significantly advance accelerator R&D and provide important experimental opportunities in high energy electron-positron physics in the second half of the 1980's. However, only a relatively small number of experimental groups can be accommodated at SLC.

We conclude that the U.S. High Energy Physics Program, as outlined here, is strong and will be capable of meeting the challenges of the 1980's, but it is not without dangers. Through the middle of the 1980's the U.S. will have facilities that are competitive with those in other regions, and, with adequate utilization, will sustain an experimental program of high quality. The Tevatron I and SLC projects, along with necessary advanced accelerator R&D studies discussed below, will provide the technological basis for the U.S. to plan the major new facilities that will be required for the field to progress in the late 1990's. Such facilities may mean multi-TeV (5-20) proton rings of great size, or an electron-positron collider, probably linear, with center-of-mass energy approaching 1 TeV. New innovative technology and the physics learned in this decade may suggest other directions for the 1990's.

It is clear to us, however, that the period of the late 1980's is especially critical to the long term health of high energy physics in this country. The world-wide competition, particularly in Europe, appears to be extremely strong during this period with the start-up of LEP and possibly HERA. Furthermore, Japan will be commissioning TRISTAN, and the Soviet Union should be starting to run UNK. The U.S. Program has no provisions for any facilities that would compete directly with any of these foreign machines.

For U.S. high energy physics to remain vital beyond this decade into the era of the super-machine of the 1990's, we must maintain an active experimental program that is competitive with the rest of the world. The construction of a new facility by the end of the decade that provides major forefront experimental opportunities for U.S. physics will allow the U.S. program to remain preeminent.

The Subpanel has tried to devise the best strategy for the U.S. High Energy Physics Program. It has considered in some detail the most desirable programs which could be implemented at two budget levels, nominally \$440M and \$395M per year for DOE and correspondingly \$35M and \$34M for NSF (FY 1982 dollars). (These levels are, respectively, 4 percent above and 8 percent below the floor funding level of the 1978 DOE/OMB Agreement for high energy physics.) The program is currently operating at a level well below the \$(440,35)M figure. With respect to this we therefore interpreted it as our charge to describe a long range plan based on an approach to this level, as specified in more detail below. For the \$(395, 34)M budgetary figures our discussion assumes a steady state at that level over the decade. Finally the Subpanel has also considered the impact of a yet lower budget level of \$360M for DOE and \$32M for NSF per year.

In the discussion to follow, for the \$440M level (Subsection B) our comments focus especially on ISABELLE. This is so because, for the lower \$395M funding level, we conclude that the ISABELLE project (in its present scope) has to be abandoned. Those elements of the \$440M level program which we recommend for retention at the \$395M level will be discussed more fully in the Subsection C dealing with that level. We also discuss those new construction projects, of more limited financial scope than the current ISABELLE plan, that might be undertaken in the later half of the decade.

B. Program at the $(440, 35)M Level

1. Recommendations at the $(440, 35)M Level

At the $(440, 35)M level, the U.S. High Energy Physics Program should contain the following essential components. The order of the following list is that of our Interim Report and is not to be construed as representing priorities.

--Construction of ISABELLE at Brookhaven with completion by the end of this decade.

--Adequate utilization and maintenance of existing accelerator and storage ring facilities, and support of important non-accelerator particle physics projects.

--Expeditious completion of the Energy Saver at Fermilab.

--Implementation of the Tevatron (TeV II) fixed-target program at Fermilab, in all three experimental areas.

--Completion of the Tevatron I (TeV I) proton-antiproton collider at Fermilab, including the provision of a major detector facility.

--Continuation of the major R&D effort at SLAC on the Stanford Linear Collider (SLC), with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for expanded physics research.

--Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating RF structures, superconducting RF cavities, and novel means of acceleration.

2. Comments on the Recommendations at the $(440, 35)M Level

These comments assume that while the immediately foreseeable budgets are likely to be below the $(440, 35)M level, this level is attained by FY 1984, or FY 1985 at the latest. The alternative situation, in which budgets remain lower, is discussed in Subsection C. We comment here mainly on ISABELLE. The remaining items are all included at the lower budget level of Subsection C, and comments on them are deferred until then.

--ISABELLE is the largest American high energy facility construction project during the 1980's, and represents a major new center for experimentation in the U.S. beginning around 1989. As a dedicated facility, it will permit a diverse broad-based research program, which can support six major experiments simultaneously. The facility is unique because of its combination of high energy and high luminosity. The high luminosity is necessary both for large statistics studies of such particles as intermediate vector bosons and for searches for rare processes and new particles. ISABELLE has a high potential for exploratory work which could lead to discoveries in the multi-hundred GeV mass range. Further, the growth potential is excellent, with electron-proton collisions and heavy ion collisions possible. It will complement both the large electron-positron and electron-proton facilities that are likely to exist in Europe at that time and also the other facilities in the U.S. Program.

The Subpanel has reviewed the ISABELLE Project in the areas of physics, technology, management, cost and schedule. As a result of the technical evaluation, it concludes that, except for superconducting magnets, the overall design is a conservative one, appropriately using many of the technical features of the highly successful CERN-ISR. The goals of ISABELLE, including the high luminosity of about 10^{33} cm^{-2} sec^{-1}, should be attainable.

With regard to the superconducting magnet design, new R&D models using the Fermilab-Rutherford cable have performed extremely well and the new design successfully addresses the key technical issues. More magnet R&D is required, however, and BNL has developed a plan to accomplish it.

Recent changes in management have been effective, but additional management changes, including the recruitment of a strong Project Director, will be required for effective program execution.

The total remaining ISABELLE cost, beginning in FY 1983 (including construction, R&D, a modest complement of detectors, and pre-operation) is estimated to be $500M (FY 1982 dollars). The average yearly support levels of $440M DOE and $35M NSF include the ISABELLE costs and a lean level of support for the remainder of the program. Since we are describing a plan based on an approach to the above funding level, we recognize that some retrenchment of the ISABELLE activity is required for the near future. For that interim period we, therefore, recommend a concentration devoted chiefly to R&D on the basic magnet, magnet production development, and magnet string tests.

It must be emphasized that at this overall budget level the funds available for ISABELLE detectors are quite modest. Furthermore, this plan does not include other desirable facilities such as a major electron-positron facility or a major electron-proton collider.

In summary, the Subpanel reaffirms the recommendation of the 1977 and 1980 HEPAP Subpanels and strongly recommends that the ISABELLE Project be built and completed during this decade. This investment in the program will enable the U.S. to maintain an active and healthy high energy physics program near the end of the decade and into the 1990's.

C. Program at the $(395, 34)M Level

This Subsection deals with the situation in which funding over the decade is assumed to be constrained to the $(395, 34)M level. The primary distinction between this and the higher level discussed above is the ISABELLE project for a high luminosity, high energy proton-proton collider. Our conclusion for the lower funding level is that construction of this scientifically valuable facility would have to be abandoned in its present scope. Between a steady $(395, 34)M level and the higher level, which assumes an early approach to $(440, 35)M, there is a difference of opinion within the Subpanel as to where the line should be drawn for abandonment of the ISABELLE project. Indeed, some members felt that inclusion of ISABELLE construction even at the higher $(440, 35)M level would jeopardize funding for the other elements of the national program. Others felt that the Project must be sustained even at the $(395, 34)M funding level. The majority was of the opinion that it should be included at the higher level and not at the lower. The Subpanel strongly believes that a new facility, more modest in cost, should be completed by the end of the decade.

1. Recommendations at the $(395, 34)M Level

--Adequate utilization and maintenance of existing accelerator and storage ring facilities, and support of important non-accelerator particle physics projects.

--Expeditious completion of the Energy Saver at Fermilab.

--Implementation of the Tevatron II (TeV II) fixed-target program at Fermilab, in all three experimental areas.

--Completion of the Tevatron I (TeV I) proton-antiproton collider at Fermilab, including the provision of a major detector facility.

--Continuation of the major R&D effort at SLAC on the Stanford Linear Collider (SLC), with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for expanded physics research.

--Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating RF structures, superconducting RF cavities, and novel means of acceleration.

--With the present ISABELLE project terminated, a start by the mid-1980's on a new high energy construction project of more modest financial scope. Although it is premature to recommend a specific direction at present, the Subpanel is aware of a number of interesting concepts currently being considered by the community and urges intensified discussion, development, and R&D aimed at selection and authorization by the middle of the decade.

2. Comments on the Recommendations at the $(395, 34)M Level

--Utilization of present facilities. At present the U.S. high energy physics experimental effort is accomplished at Fermilab with its 400 GeV proton facilities, at SLAC on the 15 GeV electron-positron PEP facility, the 3.5 GeV electron-positron SPEAR facility and the 25 GeV electron fixed-target facility, at Cornell on its 8 GeV electron-positron CESR facility, and at BNL with its 30 GeV proton fixed-target facility. Utilization of these accelerators and storage ring facilities provides the lifeblood of the university program in high energy physics, and maintains the flow of outstanding young scientists into the field. This flow is essential to the continued preeminence of the U.S. High Energy Physics Program. In the last several years relatively low operating funding, combined with the R&D needs for approved construction projects, has led to an inadequate utilization level of existing facilities. For example, both at Fermilab and Brookhaven, the number of accelerated protons has dropped to about half the maximum levels reached in 1979. Important experiments are not performed, are deferred or stretched-out unreasonably, or are terminated prematurely. The Subpanel recommends more support for accelerator operations and the physicists who use these accelerators. Every increase in support has an extremely high leverage in terms of increased operations. In the latter half of this decade when the new facilities begin operations, the program on older facilities such as the AGS and the SLAC fixed-target will be decreased or terminated in order to fund operation of the new facilities.

As discussed in more detail in Section VI, a modest fraction of high energy funds should go into the support of important non-accelerator particle physics projects.

--The expeditious completion of the Fermilab Energy Saver together with utilization of existing facilities, remains the highest priority of the U.S. Program. Its construction is well underway and operational trials are scheduled for 1983. Not only will this device be the world's first superconducting synchrotron, it is also the prerequisite facility for the completion of TeV I and TeV II discussed below. Together, these Tevatron facilities will become the backbone of the university user program. This new accelerator, with the world's highest energy throughout the 1980's, is also a major advance in accelerator technology. The successful operation of the Energy Saver will give confidence that future superconducting-magnet accelerators and storage rings can be built. Moreover, the Energy Saver will provide enough economy in electric power consumption to permit year-round operation of the accelerator.

--TeV II, with secondary beams of energies up to 1 TeV, will be the world's major center for fixed-target experimentation through the 1980's. It will open a new energy region, and provide broad opportunities for many kinds of experiments. These include studies of the quark structure of a variety of hadrons, searches for the tau neutrino and neutrino oscillations, studies of hadrons containing heavy quarks, and investigation of a variety of rare phenomena including CP violation.

--The TeV I proton-antiproton collider with 2 TeV available total energy represents an exciting step of relatively modest cost to a new frontier in hadron-hadron physics. Throughout the decade and into the 1990's, TeV I will attain the highest center-of-mass collision energy of any facility in the world. Important experiments will include studies of both the charged and neutral intermediate bosons W and Z^0, searches for heavier such particles, and searches for composite structure of the basic constituents of matter. This project will advance accelerator technology in a number of ways and provide invaluable experience in the cooling of high energy particle beams.

--SLC construction serves two purposes. First, SLC represents a dramatic advance in the field of particle accelerators, which provides the means for a thorough investigation and demonstration of the technology of the collision of micron-size electron and positron beams. Such an investigation is a crucial step in the design of very high energy linear colliders for the next decade. The second purpose is to carry out the early exploration and systematic study of the very rich and important Z^0 decay modes and of the associated electroweak physics. The Subpanel feels that it is important that these purposes be achieved as early as possible, and therefore, recommends construction authorization in FY 1984.

The Subpanel recognizes that SLC requires the use of novel accelerator technology and the adaptation of a linear accelerator not built for that purpose. Therefore, this construction authorization (about $80M) should be limited to the conventional facilities, beam manipulating systems, beam transport systems, positron source and other items required to obtain beam-beam interactions in the 100 GeV range at one interaction point. This will allow comprehensive studies to be made of the performance characteristics of this exciting new accelerator technology, and the luminosity is expected to be sufficient for early studies of Z^0 physics. The total cost for SLC R&D, including construction work and associated operating activity is $120M (FY 1982 dollars) beginning in FY 1983, with construction completion in 1986, assuming authorization in FY 1984.

The expansion of the physics capability of SLC with a second interaction region (with its associated beam transport systems) and extensive new detector construction will require an additional expenditure of about $50M. The authorization of that expenditure should be paced by the successful progress of the R&D work under way. The major measure of that progress is that on the basis of this R&D a luminosity of at least 10^{29} cm^{-2} sec^{-1} can be reached with reasonable confidence. This will allow the study of 50,000 Z^0 decays per year. (The luminosity expectation of 6 x 10^{30} is expected to yield 3 million Z^0 decays per year.)

--Advanced accelerator R&D must remain an essential component of the U.S. Program to provide the tools and techniques which will make the accelerators of the future technically and economically feasible.

In particular, the Subpanel endorses the recommendation made by the "Tigner Panel" ("Report of the Subpanel on Accelerator Research and Development of the High Energy Physics Advisory Panel," June 1980) that advanced accelerator R&D should be funded at a level of effort up to about 4 percent of the U.S. High Energy Physics Program, which is a significant increase over the present level of effort.

As already pointed out, both the TeV I and the SLC projects have strong R&D components which indeed provide part of the justification of these efforts. There are, however, other R&D areas which must be urgently pursued. The development work on superconducting RF cavities at Cornell should be continued and expanded in scope. This outstanding work opens the possibility of constructing a more cost effective future electron-positron facility. Other R&D areas with important potential payoff for the future include high field superconducting magnets, high gradient accelerating structures, cooling techniques, and novel methods of acceleration.

There is the need to hold workshops on special topics in accelerator physics, and also to teach and train graduate students, postdocs, and young scientists. Most importantly, there is the need to examine accelerator systems which are not constrained by location, size, geology, and existing facilities of the present centers.

--A new accelerator project, whose design and construction should be initiated by the mid-1980's based on earlier development work, is a crucial aspect of this program. Without ISABELLE, the completion of a new accelerator capability by the end of the decade is important for several reasons:

a) By 1990 most of the older accelerators will have been in use for at least a decade, the range of new physics they can illuminate may be shrinking, and there will be important scientific questions that cannot be addressed by these machines.

b) At the TeV I and SLC facilities, the number of available interaction regions will be limited.

c) Opportunities for adequate University involvement in high energy physics will need expansion.

A number of new and exciting facility ideas are now being studied by the community, and still other schemes are likely to emerge in the near future. We believe that the time will be ripe for selection and authorization of a construction project by the mid-1980's. In preparation for this, we urge that intensified discussion, development, and R&D begin as soon as possible.

These ideas are still in preliminary form for the most part, and the Subpanel has therefore not attempted to make choices among them. Some examples of new projects of appropriate scale under current study in the community include: an electron-proton collider or a less expensive high luminosity ($L \sim 10^{33}$ cm^{-2} sec^{-1}) hadron-hadron collider built in the ISABELLE tunnel; a second proton collider ring at Fermilab dedicated to proton-antiproton, proton-proton, and/or electron-proton collisions; an electron-positron collider using superconducting RF cavities (as proposed for CESR II); or a combination of smaller facilities, one of which might be a major non-accelerator facility. Many of these projects are represented by a considerable R&D effort at present (for example, that now devoted to ISABELLE at Brookhaven or CESR II at Cornell). It is important that these efforts be encouraged to continue, and, in some cases, be increased and diversified.

It appears likely that the cost of such a project may be in the range of \$250-\$300M. If the cost were higher, its completion would be delayed well into the 1990's with two disadvantages: (i) corresponding delay in its exploitation at a time when other facilities were being retired, and (ii) possible delay in the initiation of a large new ultra-high energy facility in the beginning of the 1990's.

3. Impact of the \$(395, 34)M Level

In conclusion, the program at the \$(395, 34)M level is a substantial one. The opportunities for experiments provided by the presently existing facilities and those which will be completed in the next several years will maintain the momentum in the U.S. High Energy Physics Program until late in the 1980's. Near the end of the decade, however, the absence of a major new facility such as ISABELLE will be apparent. A modest new facility will help to fill the gap, but will necessarily provide more modest opportunities than would ISABELLE, and would probably turn on later. In the next decade, the U.S. effort, while of high quality, would be less competitive.

D. Impact of Funding the High Energy Physics Program at \$(360, 32)M Level

To be responsive to the Charge, the Subpanel presents here a brief impact report on a yet lower level, equivalent to a \$(360, 32)M level of funding. As outlined above, a reduction from \$440M to \$395M in DOE funding would require the termination of the ISABELLE project so as to preserve reasonable levels of utilization of existing facilities and to insure some flexibility in research and development work leading to vital future facilities. An additional reduction to a spending level of \$360M per year would lead to further attrition of physics output, personnel, and the ability to carry out competitive research in future years.

Such a level of funding continued through this decade would have the following impact:

1. Termination of the ISABELLE project.
2. Impossibility of a new initiative in the mid- to late-1980's as included in the \$395M budget.

3. One or more of the following actions: early termination of the AGS and SLAC fixed-target programs; abortion of the SLC project; termination of the TeV I collider project; sharp reductions in utilization at all laboratories.

These actions would have very serious negative implications for the maintenance of a successful high energy physics program. Termination of both ISABELLE and the AGS would leave Brookhaven without any high energy physics facility. Failure to construct the SLC would eventually have a similar impact on SLAC. Moreover, it would close off one of the important avenues to very high energy physics by preventing essential research for the linear collider concept. Termination of the TeV I project after the elimination of ISABELLE would mean the total abandonment of very high energy hadron physics in this country. Finally, any further reductions of utilization would bring high energy experimental physics to a near stand-still. Many physicists would undoubtedly leave the field and this irretrievable loss of competent and highly trained personnel would perhaps be the most damaging result. The result of the actions outlined above would represent a staggering blow to the health of the U.S. High Energy Physics Program. The program would be headed for a serious decline. It is important to emphasize that the present ongoing program is living on the construction investments of the past. If such investments are sharply curtailed in the present, the future will be bleak. Once lost, the preeminent U.S. position could be restored, if at all, only after many years of improved support.

SECTION VIII

INTERNATIONAL COLLABORATION

Science has always benefitted from the widest possible exchange of ideas, information, and experience and this is particularly true in the field of high energy physics. International exchanges occur through conferences, publications, and visits but are even more effectively realized when physicists work in the institutions of other regions. The exchange of physicists and groups can allow a more diversified program of physics in each region, unless the complete range of accelerator facilities can be provided on a national or regional basis. As the cost of high energy machine: increases, the feasibility of such provision for each country or region diminishes and the international use of those facilities that can be provided become highly desirable. The alternative would be a narrow physics specialization in each country which would inevitably stunt the advancement of the field. As long as such exchanges go in both directions, they represent little real increase in cost to any one country, and the benefits are shared by all. They should clearly be encouraged.

Cooperation across national boundaries has so far taken a wide spectrum of forms:

a. Individuals from one country working at laboratories in other countries for periods of a year or more, supported either by their home institution or by the host institution. This liaison has been especially strong between the U.S. and Europe, in both directions, but also exists to a lesser degree with Japan and the USSR.

b. Groups from one country performing experiments at a facility in another country, with or without local collaborators. Some examples within the U.S. program are the American groups at the CERN-ISR, and at PETRA, and the various European university groups at Fermilab, SLAC, and Cornell. Such collaborations, motivated by the physics interests of the scientists involved, have formed in much the same way as those which do not cross national boundaries. Program committees at the major U.S. and European laboratories have generally approved experimental proposals on their scientific merit without regard to the national affiliations of the proposers. In January 1980 a working group of U.S. and European physicists (Mulvey, Peoples, Strauch, and Weber) surveyed the record of such collaborations over the 1974 - 1980 period and concluded that "The collaborations between the U.S. and Western Europe have made significant contributions to the success of the programs in each region. There was a very good balance in the efforts in each direction during the period investigated."

c. Collaborations involving major transfers of equipment or funds between countries. These are usually negotiated between the responsible Government agencies. Examples include the Japanese participation at Fermilab, SLAC, and BNL, the Soviet participation at Fermilab, and the French participation at Serpukhov.

d. Collaborative accelerator construction projects. The most conspicuous example of successful international cooperation in accelerator construction is the series of large facilities including the PS, the ISR and the SPS built at CERN through the joint effort of 12 European countries.

Because of the enormous costs of the ultra-high energy facilities which may be needed in the future, the notion of collaborative accelerator construction projects will probably have to be extended to a world-wide collaboration to build machines beyond the means of any one country or region. The International Committee for Future Accelerators (ICFA) with representatives from the USSR, Western Europe, Japan, and the U.S. has been the group in which this issue has been considered and discussed. In its November 1981 meeting, ICFA recommended the setting up of two task forces, one to study and design a 20 TeV proton accelerator and the other to study and design an electron-positron collider of at least 300 GeV energy per beam. ICFA's stated aim is to start construction of one or both machines in about 10 years.

The Subpanel has considered the whole area of international collaboration including the ICFA recommendations and submits the following recommendations.

A. Recommendations

1. Laboratories and funding agencies should help support participation of individuals and groups in experiments at facilities not available in their own countries.

2. The U.S. funding agencies should help to support outstanding joint ventures at CERN, DESY, or other non-U.S. high energy laboratories to provide capabilities not available in the U.S. In the other direction, U.S. laboratories should also be receptive to such projects.

3. The U.S. should entertain the possibility of major bilateral accelerator construction projects with Europe or Japan, if they are cost effective and have strong physics motivations.

4. The Subpanel supports collaboration with the ICFA initiative in laying the groundwork for the large preparatory effort required before a world-wide super-accelerator can be built. However, the Subpanel does not recommend the commitment of significant U.S. resources to the ICFA project at the present stage of international collaboration.

B. Comments on the Recommendations

1. This form of international cooperation has been active for some time. SLAC, DESY, and Cornell have long had foreign representatives as members of their program committees, and a similar policy might also be considered at Fermilab, CERN, and Brookhaven.

2. Examples of the joint ventures under consideration would include the construction of major detectors, or the equipping of new experimental areas.

3. In the construction of accelerator facilities beyond the means of any one country or region, bilateral arrangements might be a natural step between the present regional facilities and a world-wide collaborative construction effort.

4. In the further future, perhaps in the 1990's, a world-wide super-accelerator project of the sort advocated by ICFA may become attractive. Such a project would require many years of administrative and scientific preparatory work. The Subpanel supports collaboration with the ICFA initiative to assist in laying this groundwork. Such collaboration may also stimulate new initiatives in accelerator R&D. However, until international collaboration has advanced considerably along the lines mentioned above, the Subpanel does not believe it is appropriate to commit significant U.S. resources to the ICFA projet.

In the context of Recommendations 1 and 2, examples of specific accelerator facilities which will be unique and therefore especially appropriate for world-wide use include the following:

a. Fermilab Tevatron II complex which will, for some time, provide the highest energy fixed-target collisions. It can and does provide good opportunities for non-U.S. users.

b. The ISABELLE facility which, if constructed, will be unique in the world and could provide good opportunities for foreign participation, particularly in view of the two experimental areas that are not initially to be developed.

c. The LEP facility which will produce the highest electron-positron collisions. The four initially undeveloped areas will likely attract U.S. experimental groups.

d. The HERA facility which, if built, would offer the highest enegy lepton-hadron collisions. This project is now actively seeking involvement by other countries.

e. The TRISTAN electron-positron (and eventually electron-proton) collider facility in Japan, which may also be appropriate for participation by outside experimental groups.

SECTION IX

LONG-RANGE PLANNING

Long range planning for the U.S. High Energy Physics Program involves the complicated interplay of many different groups including the funding agencies, the accelerator laboratories, the community of experimental and theoretical high energy physicists, and advisory bodies such as HEPAP and Subpanels to HEPAP. The most visible part of this planning process in recent years has been the series of HEPAP Subpanels arranged on an ad hoc basis to make recommendations on construction of new accelerator and storage ring facilities and to consider program priorities under specific budget assumptions. The present Subpanel is the fifth in the recent series that began with the Weisskopf Subpanel in 1974.

It is evident to this Subpanel that there are a variety of concerns in the high energy physics community about present methods of long range planning. For example, there is some feeling that the high energy community does not have adequate opportunity to participate in this planning process which so affects its future. Another difficulty is the short time scale available for reaching decisions on complicated issues. Finally, there seems to be a clear need to inform better the entire community about the issues related to long range planning and facility construction.

The above list of concerns is certainly not complete but it does point out the need for improvement in the procedures for long range planning. It is essential, of course, to stress that no committee or other planning structure, however wise its membership, can be a substitute for the creativity that must come from individuals within the field. Such committees can react to proposals, can help create the climate in which scientists can get together and generate new ideas, and can attempt to rank the relative merits of competing proposals. They cannot by themselves generate new projects. New initiatives must come from the people who are going to do the work. It is natural and commendable that the national laboratories with large accelerator expertise are the places where much of the creative work in the development of new facilities is done.

For purposes of short-term planning, the HEPAP system seems to have worked well. Its reputation in the Government is good; outstanding scientists have served on it, and it has been led by distinguished Chairmen.

The Subpanel has discussed at some length possible ways to improve the process of long range planning. One possible planning mechanism seriously considered was the formation of a joint HEPAP-DPF standing committee on long range planning whose functions might include: (1) the sponsorship of DPF workshops on new facilities or on the exploration of new technologies relevant to detectors or accelerators; (2) the issuance of occasional reports on the state of high energy physics and future planning; and

(3) the preparation of specific reports for HEPAP and the funding agencies as requested. Thus, part of the function of such a committee, perhaps augmented by some ad hoc members, might be to carry on the studies presently done by the ad hoc Subpanels on facilities and on long range planning. The advantages of having such a group would include the greater DPF involvement and interaction, the greater continuity of the planning activity, and the broadening of the issues considered.

In spite of these good features, the Subpanel could not convince itself that these advantages over the existing systems were sufficiently compelling to justify recommending at this time that a new formal structure be set up. HEPAP and the DPF Executive Committee may wish to consider in more depth than has been possible for the Subpanel whether such a standing committee might be an appropriate substitute or addition to the present ad hoc groups. Aside from this structural question, it would seem appropriate to the Subpanel if DPF on its own made a larger effort in the planning area. This effort might involve such activities as sponsorship of appropriate workshops as already discussed and increased scheduling at its meetings of invited talks in areas relevant to future planning. Indeed DPF may wish to set up on its own (without HEPAP or funding agency involvement) a small committee specifically charged with the implementation of such planning activities. The existence of such a group, if effective, might lead in an adiabatic way to a full-fledged committee on long range planning such as that mentioned earlier.

APPENDIX A

CHARGE
SUBPANEL ON LONG RANGE PLANNING FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM

Within the context of changing worldwide high energy physics activities and opportunities, review the status and prospects of the U.S. program (DOE & NSF) taking into consideration all aspects of the program.

Develop a strategy and long-range plan for the U.S. Program over the next decade under the following funding constraints:

1. The DOE/OMB Agreement of 1978 which is equivalent to a DOE level of $423M in FY 1982 dollars. The NSF FY 1979 level of $25M for elementary particle physics is equivalent to $35M in FY 1982 dollars.
2. A level of 10 percent lower.
3. A level of 10 percent higher.

Within each of the above scenarios assess the prospects and sufficiency of the strategy and long-range plan to maintain a forefront U.S. High Energy Physics Program.

In view of the pivotal role which the ISABELLE project has on long range planning please submit an interim report which evaluates the priority of ISABELLE in each of the three constraint cases, taking into consideration the physics potential of the facility, the funding and schedule required to complete construction, and competing needs and opportunities.

The interim report should be submitted by November 1, 1981, and the report should be completed by January 1, 1982.

APPENDIX B

Membership and Consultants

Subpanel on Long Range Planning for the U.S. High Energy Physics Program

Membership

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B. Barish - California Inst. Tech.
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J. Bjorken - Fermi Nat. Acc. Lab.
J. Cronin - U. Chicago
H. Feshbach - Mass. Inst. of Tech.
V. Fitch - Princeton U.
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R. Palmer - Brookhaven National Laboratory
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H. Politzer - California Inst. Tech.
P. Reardon - Princeton Plasma Physics Lab.
J. Rosner - U. Minnesota
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A. van Steenbergen - Brookhaven National Laboratory
P. Wilson - Stanford Linear Acc. Center

INTERIM REPORT OF THE SUBPANEL ON LONG RANGE PLANNING FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM OF THE U. S. DEPARTMENT OF ENERGY HIGH ENERGY PHYSICS ADVISORY PANEL

NOVEMBER 1981

U.S. Department of Energy

High Energy Physics
Advisory Panel

November 3, 1981

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
ER-1:FORSTL
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Washington, D.C. 20585

Dear Al,

I am transmitting to you the Interim Report of the 1981 HEPAP Subpanel on Long Range Planning in High Energy Physics chaired by Dr. George Trilling of UC-Berkeley. HEPAP discussed this report at its meeting in Washington, D.C. on November 1, 1981, and supports its recommendations unanimously.

In this Interim Report the HEPAP Subpanel was asked to evaluate the priority of ISABELLE "taking into consideration the physics potential for the facility, the funding schedule required to complete construction, and competing needs and opportunities." Its strong recommendation "that the ISABELLE project go forward and be completed during this decade" is an endorsement of the diverse and exciting physics potential that this major facility will provide.

The Subpanel identified two specific and important funding requirments for the construction of ISABELLE:

1. Funding must be sufficient for ISABELLE completion within this decade.

2. Funding must be sufficient to support adequately six essential components of the national program, whether or not ISABELLE is constructed. The six components are:

    i. adequate utilization of existing facilities and support of user groups

    ii. expeditious completion of the Energy Saver at Fermilab and initiation of the Tevatron II (fixed target) program

    iii. completion of the Tevatron I antiproton-proton collider

    iv. R&D work at SLAC leading to the SLC linear collider project

    v. development work on superconducting rf cavities at Cornell

    vi. accelerator R&D on high field superconducting magnets, high gradient accelerating structures, and novel methods of acceleration.

If the two funding requirements cannot both be met, then the ISABELLE project cannot be continued.

From these program requirements, the Subpanel identified a minimum annual level of support that averages $440 M (FY 1982 dollars) in DOE funds per year (plus approximately $35 M of NSF support) for the high energy physics program. HEPAP endorses this recommendation with the proviso that substantial progress toward the $440 M funding level (in FY 1982 dollars) be achieved in FY 1983 and that the Administration make a commitment to support the national program at the $440 M level in FY 1984 and beyond.

A national high energy physics program that includes ISABELLE and fulfills the six conditions listed under Item 2 above is exciting, will remain active into the 1990's, will address many of today's central questions in physics, and will provide many opportunities for major discoveries. It is a program that will maintain U.S. preeminence in the face of strong international competition.

If the identified funding minimum of $440 M (FY 1982 dollars) cannot be achieved, then HEPAP supports the Subpanel recommendation that the ISABELLE project cannot be continued. Without ISABELLE, but with a budget adequate to meet the six essential components in Item 2, the U.S. will have a viable program for most of this decade. However, by the end of the decade, there will be important but relatively limited opportunities for experimentation at the high energy frontiers. The ability of the U.S. program to attract and hold the best young researchers will be sharply reduced.

We have considered the cost of such a minimal national program as defined by these six essential components alone and have come to a figure of $395 M (FY 1982 dollars). This figure allows for adequate short term utilization and would alleviate the current problems of severe underutilization of existing facilities and inadequate support for the users. It also provides for the advanced accelerator R&D effort vital for future accelerators at the high energy frontier. But the loss of ISABELLE would be a severe blow to the strength and diversity of the U.S. program in elementary particle physics in the late 1980's and the 1990's.

Sincerely,

Sid

Sidney D. Drell
Chairman, HEPAP

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October 30, 1981

Dr. Sidney Drell, Chairman
High Energy Physics Advisory Panel
Stanford Linear Accelerator Center
P. O. Box 4349
Stanford, CA 94305

Dear Sid:

I am submitting herewith the Interim Report of the 1981 HEPAP Subpanel on Long Range Planning in High Energy Physics. This Interim Report attempts to be particularly responsive to those issues in the charge for which a response by November 1 was required. The task of our Subpanel is not completed, and we do hope to have a final report available by the end of this calendar year.

Sincerely,

George H. Trilling

George H. Trilling

Enclosure

GHT:cfd

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SECTION I

PREFACE

The High Energy Physics Advisory Panel (HEPAP) Subpanel on Long Range Planning was formed in August 1981 to advise on the U.S. High Energy Physics Program. The Charge to the Subpanel is given in Appendix A. In response to a 12 percent cut in budgetary support for High Energy Physics for FY 1982 proposed in late September 1981, Dr. J. Leiss of DOE after discussion with HEPAP on September 28 requested that the Subpanel also consider a funding level 10 percent lower than the lowest one in the Charge.

The Subpanel held an initial organizational meeting on September 4 - 5, 1981 in Washington, D.C. On September 24 - 26, 1981, the Subpanel met at DOE Headquarters in Germantown, Maryland, and heard presentations from representatives of the high energy accelerator laboratories--Cornell, SLAC, Fermilab, and Brookhaven--and from various other individuals. The Subpanel has since held further meetings on October 9 - 10, 1981, at Brookhaven and October 17 - 19, 1981, at DOE Headquarters in Germantown, Maryland. A Technical Subgroup of the Subpanel with various consultants made further visits to Fermilab (October 1 - 2) and to Brookhaven (October 8 - 9) to assess the technical status and the anticipated schedule and costs for the Tevatron I (Tev I) project at Fermilab and the ISABELLE project at Brookhaven. The list of Subpanel members and consultants who participated in these visits is given in Appendix B. To solicit input from the high energy community, a letter from the Subpanel Chairman inviting such input was sent to each member of the Division of Particles and Fields of the American Physical Society. By the time of the October 17 - 19, 1981 meeting about 168 responses to this invitation representing the views of 234 people had been received, and were distributed to all members of the Subpanel.

The present document is the interim report, requested in the Charge, evaluating the priority of ISABELLE for the various constraint levels. It is important to emphasize that indeed this is just an interim report and that the work of the Subpanel is not complete. Technical Subgroup visits to SLAC and Cornell will be made in November 1981, and further deliberations, now scheduled for early December 1981 will be required to produce the final report. At this time the Subpanel has attempted to assemble as much scientific, technical, and budgetary input as possible relevant to the issue of ISABELLE priority.

The present Subpanel is the fifth in a series of recent Subpanels (1974, 1975, 1977, 1980) which have reviewed the status of the U.S. High Energy Physics Program and made recommendations for future directions. Both the ISABELLE project and the colliding beam effort at Fermilab were recommended by the 1977 Subpanel. The anticipated funding level on which these recommendations were based was higher than the highest level of the present Charge: namely, 10 percent above the DOE/OMB Agreement of 1978. However, the 1977 Subpanel did consider a lower level, closely equivalent to the intermediate level of the present Charge (the DOE/OMB Agreement of 1978), still recommended the ISABELLE project, but stated that "this funding level on a long term basis is close to that which would require a drastic revision in the long range program and probably is not adequate to keep the U.S in a world competitive position in this field."

The present Subpanel has reexamined the status of ISABELLE, considering scientific, technical, and budgetary issues. The recommendation and its justifications are given in Section II (Recommendation and Discussion). Discussion of the Scientific Issues is in Section III, and the Technical Assessment is in Section IV.

SECTION II

RECOMMENDATION AND DISCUSSION

A. Recommendation

The Subpanel strongly recommends that the ISABELLE project go forward and be completed during this decade. ISABELLE provides a major new facility that will enable the U.S. High Energy Physics Program to remain active and healthy during this decade and into the next. ISABELLE construction and the other essential components of the U.S. Program require a minimum annual level of support that averages $440M (FY 1982 dollars) in DOE funds per year. If support at this level cannot be made available in time for ISABELLE completion within this decade, the ISABELLE project cannot be continued.

B. Comments on the Recommendation

This interim report is submitted to provide recommendations on the ISABELLE project under various funding levels. The Subpanel has reviewed the existing programs and future plans at all the major high energy physics laboratories. Considering the physics objectives discussed elsewhere in this report, it has assessed the role of ISABELLE and has considered carefully the program, exclusive of ISABELLE, which must be maintained during the years of its construction. This partial program consists of actively exploiting existing facilities, finishing construction of other new facilities, and doing R&D that will create techniques to reach new frontiers in the field. This program plus the costs for ISABELLE require an average funding for high energy physics during the ISABELLE construction of $440M per year from DOE and $35M per year from NSF (FY 1982 dollars). Support at this level will provide a lean, but healthy U.S. Program in high energy physics over the coming decade. The essential components of this Program, which must be supported whether or not ISABELLE is constructed, are the following:

1. Adequate utilization of existing facilities. The recent erosion of operating funds, when combined with the needs for R&D for approved construction projects, has led to an inadequate level of utilization of existing facilities. Important experiments are not performed or are prolonged unreasonably. More support for accelerator operations and the physicists that use these accelerators is required. Support for large non-accelerator particle physics projects is becoming increasingly important.

2. The expeditious completion of the Energy Saver at Fermilab and initiation of the Tevatron II (Tev II) program. Tev II will be the major center for fixed target experimentation through the 1980's. It opens a new energy region and provides broad opportunities for many kinds of experiments.

3. Completion of the Tev I proton-antiproton collider. Tev I represents an exciting step to a new energy frontier in hadron-hadron physics. This enhancement of existing facilities will help keep U.S. high energy physics at the forefront of the field.

4. R&D work at SLAC leading to the SLAC Linear Collider (SLC) project. This effort is extremely important in developing techniques needed to reach super-high energy in electron-positron physics (e.g. 350 GeV x 350 GeV colliding electron linacs) for the long range future. Moreover, SLC presents an attractive and inexpensive means for building an electron-positron project in the Z^o energy region.

5. The development work on superconducting cavities at Cornell. This work is of outstanding quality and opens the possibility of constructing a more cost effective future electron-positron facility.

6. Accelerator R&D on high field superconducting magnets, high gradient accelerating structures, and novel methods of acceleration.

The program described above is exciting and will remain active through the decade. It addresses many of today's central questions in physics and provides many opportunities for profound discoveries. It also has the strong component of accelerator R&D necessary to the future design of more advanced machines.

This program by itself, however, is insufficient to maintain the preeminence of the U.S High Energy Physics Program through the end of this decade. Existing facilities will have matured and the major new accelerators will be too far off in the future to maintain a sufficient flow of research activity. Tev I and SLC will provide important but limited opportunities for forefront experimentation. ISABELLE represents the facility to provide a major new center for experimentation in the U.S. beginning in the late 1980's.

ISABELLE is a dedicated facility that will permit a diverse broad-based research program, supporting six major experiments simultaneously. The facility is unique because of its combination of high energy and high luminosity. The high luminosity is necessary both for large statistics studies and for searches for rare processes. ISABELLE has a high potential for exploratory work which could lead to discoveries in the multi-hundred GeV mass scale. Further, the growth potential is excellent with electron-proton collisions and heavy ion collisions possible. It will complement both the large electron-positron and electron-proton facilities that are likely to exist in Europe at that time and also the other facilities in the U.S. Program.

The Subpanel has reviewed the ISABELLE project in the areas of physics, technology, management, cost, and schedule. As a result of the technical evaluation, it concludes that, except for superconducting magnets, the overall design is a conservative one, appropriately using many of the technical features of the highly successful CERN-ISR. The goals of ISABELLE, including the high luminosity of about $10^{33}cm^{-2}sec^{-1}$, should be attainable.

With regard to the superconducting magnet design, new R&D models using the Fermilab-Rutherford cable have performed extremely well and the new design successfully addresses the key technical issues. More magnet R&D is required, however, and BNL has developed a plan to accomplish it.

Recent changes in management have been effective, but additional management changes, including the recruitment of a strong Project Director, will be required for effective program execution. A fully integrated construction project cost and schedule was not available at the time of the review but the Subpanel feels that the present cost estimates and schedule are approximately correct assuming that the critical magnet R&D program is satisfactorily completed by the end of FY 1984.

The total remaining ISABELLE cost, beginning FY 1983 (including construction, R&D, detectors, and pre-operation) is estimated to be $500M (FY 1982 dollars). The average yearly support levels of $440M (DOE) and $35M (NSF) include the ISABELLE costs and the funds necessary to support the program described in Items 1-6 above. In arriving at these figures the Subpanel has assumed a minimal level for the program exclusive of ISABELLE. It is the Subpanel's strong view that further erosion of the support of that program would produce major damage. Such damage has already been documented in the HEPAP Budget Impact Report of May 1981. It is for this reason that the Subpanel feels that if the above overall support levels cannot be maintained over a time scale which provides for ISABELLE completion by the end of the decade, the ISABELLE project cannot continue.

In summary, the Subpanel reaffirms the recommendation of the 1977 and 1980 HEPAP Subpanels and strongly recommends that the ISABELLE project be built and completed during this decade. This investment in the program is necessary to enable the U.S. to maintain an active and healthy high energy physics program near the end of the decade and into the 1990's.

SECTION III

SCIENTIFIC ISSUES

A. Status of the Field and General Approach

In the 1970's there was extraordinary progress in understanding the constitutents of matter and their mutual interaction. We note that this progress has occurred in at least four somewhat interconnected areas. First, experiments have established the existence of several new fundamental fermions: the charm quark, the bottom quark, and the tau lepton. Second, we now have the SU(2) X U(1) theory of the unification of the weak and electromagnetic interactions, this theory approaches quantum electrodynamics (QED) in its predictive power. Third, the concept of the quark-parton structure of hadronic matter has been extended enormously both by experiment and by phenomenological theory. Fourth, we now have a candidate fundamental theory for the strong interaction, quantum chromodynamics (QCD), which rivals QED in its beauty, economy of formulation, and potential range of validity. Thus, we now have what is often called the "standard model" for elementary particle physics.*

Although we recognize that some aspects of this theory need crucial experimental tests, it does provide a comprehensive reference to which alternative theories and models can be--and inevitably are--compared. The standard model also provides a way to classify the potential significance of experiments and of future facilities.

However, we must be ready for surprises. It is often said--and history bears witness to its correctness--that the reasons stated in advance to justify construction of a facility turn out not to be the most important ones. One may argue that the present situation with respect to QCD and SU(2) X U(1) is similar to the status of QED in the past; that is, they are becoming well-verified theories. Hence, one may argue that the area where surprises can occur is much smaller than it was in the past. Nevertheless, even within the standard picture, there is an additional force not clearly associated with gauge interactions. This is the so-called Higgs sector, a somewhat awkward feature of the theory. There can be every expectation of surprises occurring in that general domain of phenomena.

We therefore identify three distinct ways of viewing the future contributions of the new facilities. We may view them in the context of: (1) testing the standard model; (2) searching for and/or discovering phenomena which at present are reasonably well-defined but which either contradict or lie beyond the standard model; and (3) discovering genuine surprises, which by definition cannot be discussed in specific terms at present. The relative weight to be given these three categories is a subjective question. Based on the above discussion, we would caution that it is not obvious that the relative weight be the same as one would have given even five years ago. Beyond this cautionary statement, the question of how to choose the balance is one of scientific instinct.

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*We define the standard model to mean three, and only three, generations of quarks and leptons, SU(3) X SU(2) X U(1) gauge theories for strong, weak, and electromagnetic forces, one Higgs boson, and CP violation via complex parameters in the mass matrix. Neutrino mass and mixing, as well as proton decay, are left optional.

In the following Subsections we discuss, in turn, the ways future facilities might contribute to the field. While the focus of this report is on the role of ISABELLE in the program, we find it necessary to view the physics from an overall perspective. We shall see that from the physics point of view an optimal high energy program requires a diversity of approach, with fixed-target physics, electron-positron colliders, electron-proton colliders, proton-proton colliders, and proton-antiproton colliders all important contributors.

ISABELLE is an important component in the overall program. In addition, the physics goals of Tev I are complementary to those of ISABELLE. In Subsection E we summarize, in the light of the preceding general discussion, the ways in which ISABELLE and Tev I will provide major advances in our understanding.

B. Contributions of the New Facilities within the Context of the Standard Model

There are two central issues facing the thus far successful SU(2) X U(1) gauge theory of weak and electromagnetic interactions. The first is the discovery of the carriers of the weak force, the intermediate bosons $W^{\pm}$ and Z^{o}, and the second is the precision measurement of $\sin^2\theta_W$, the main free parameter of the model. While one anticipates the Z^{o} to be discovered at CERN well before ISABELLE is completed, the $W^{\pm}$ are more difficult to find. While the Fermilab proton-antiproton colliding-beam facility (Tev I) is expected to have this capability, the number of Z's and/or W's which can be accumulated at CERN or Fermilab is much smaller (by a factor of 100-1000) than at ISABELLE. Thus, while the intermediate boson discovery may not happen at ISABELLE, the production dynamics can be studied most incisively there. This study is not only a probe of the weak force, but is also a sensitive probe of the strong interaction at short distances.

There are many other properties of these particles such as mass, width, and decay modes into various final states, which are of vital theoretical interest. In the case of the Z^{o}, these detailed properties are most easily studied with electron-positron colliders (such as by LEP at CERN, SLC at SLAC or CESR II at Cornell). Precision measurements of the Z^{o} width yield a count of the number of neutrinos in nature. Measurements of partial widths, angular distributions, and polarization asymmetries sensitively test the structure of the weak coupling. An electron-proton collider can also make powerful, albeit indirect contributions, especially with regard to the mass and couplings of $W^{\pm}$ to quarks and leptons, which is at the very least competitive with what is obtainable from a proton-proton collider such as ISABELLE.

Production of pairs of charged W's is a very important goal, because it tests in a basic way the structure of the SU(2) X U(1) gauge theory. With the full energy of LEP, this process can be studied; indeed this has been a principal motivation for its high design energy. This process may also be accessible for study at ISABELLE and Tev I, although in either facility the experiment is difficult.

The second important goal includes the measurement of the basic parameter, $\sin^2\theta_W$, of the electroweak theory. This must be done at high and low energy scales in order to measure radiative corrections. At high energies, polarization asymmetries in Z^0 decay, as measured in electron-positron facilities such as SLC, are a sensitive probe. At low energies, a variety of facilities are of importance, especially lepton beams such as exist at Tev II, or even polarized electron-electron scattering at the SLAC linac.

If a variety of measurements of $\sin^2\theta_W$ continue to yield values consistent with the presently quoted number, confidence in the idea of grand unification of the electroweak and strong forces will be strengthened. This will in turn tend to stimulate even more the currently growing interest in non-accelerator research such as proton decay or monopole searches. It is self-evident that discovery of proton decay in the present round of experimentation would serve the same purpose, as well as providing spectacular support for the notions of grand unification.

Another significant goal involves the measurement of as yet undetermined couplings in the low energy limit of the conventional weak interactions. While this is no place for a comprehensive summary, fixed target facilities such as Tev II or the Alternating Gradient Synchrotron (AGS) which provide hyperon beams, neutrino beams, etc. are of special importance.

Parameters of the mass and mixing matrix which determine the pattern of quark weak decays also need measurement. Here again fixed target facilities such as Tev II and AGS, where subtle issues may be addressed, are essential. In the case of the heavy quarks, SPEAR remains an excellent place to study couplings of the c-quark, while CESR and DORIS are the facilities at present for the study of b-decays. Many heavy quarks are produced in the Tev II secondary beams, and in the long run these may become a very fruitful source of information as well. Semileptonic production of b-quarks--especially at an electron-proton collider--may be yet another source of information on weak couplings of the b-quark.

Investigation of the weak couplings of the t-quark depends upon its mass; for lower masses (<50 GeV) electron-positron colliders are optimal, while for the highest masses (up to 100 GeV) proton-proton and proton-antiproton colliders may provide information. Again electron-proton colliders are effective if the t-quark mass is less than 50 GeV.

In the standard model, CP violation has a specified origin in the aforementioned mixing matrix. Again a variety of facilities may contribute, notably Tev II, AGS, and CESR.

If neutrinos have masses and mixing parameters, there are yet other fundamental parameters of the theory to determine. Tev II, AGS, meson factories, reactors, and perhaps non-accelerator facilities such as underground or underwater detectors may be important contributors.

There is also, within the standard model, the task of finding the three most prominent missing particles--the tau neutrino, the t-quark and the Higgs boson. Direct detection of the tau neutrino appears to require Tev II. With respect to the t-quark and Higgs boson no one knows what masses these particles have. They could be anywhere from 10-30 GeV at the low energy end to hundreds of GeV at the upper energy end. For low masses (less than about 40 GeV) electron-positron colliders appear optimal for both Higgs boson and top quark. For higher masses (up to 100 GeV) the proton-proton and proton-antiproton colliders may be excellent tools for finding the top quark. For the highest Higgs boson masses, LEP or an even higher energy electron-positron collider may be needed.

Study of the strong interactions focuses on the further testing of QCD, the present leading candidate for a fundamental theory. Its single parameter α_s has been most incisively measured in electron-positron and lepton-nucleon collisions--not in hadron-hadron collisions. This trend may be expected to persist as the energy scale increases. But in addition to the classical tests of electron-positron total cross-section, gluon jet production, study of quarkonium properties, and of scaling violations in lepton-nucleon collisions (available in the future to electron-proton colliders), one may add gluon jet production in electron-proton collisions and, possibly, in proton-proton and proton-antiproton collisions. However, a "precision" measurement of the coupling constant is only one feature of QCD. Production of dileptons--especially those from Z^o or $W^{\pm}$ decay--and of jets of extremely high transverse momentum are excellent tools for probing QCD at the smallest distance scales. ISABELLE, with its high luminosity should not only allow observation of dileptons and jets but also allow incisive studies of jet structure. Some studies, such as direct photon or lepton production, are already easy to anticipate. Many others are certain to follow.

The properties of the strong force at larger distances--including the question of quark confinement--are a vital element as well. Many of these issues, such as the existence of gluonium, may be best studied at lower energy facilities such as SPEAR, the AGS or SLAC linac. However, others are appropriate for proton-proton and proton-antiproton colliders.

Studies of high energy soft collisions, such as measurement of elastic, total, and various diffractive cross-sections, multiplicity and correlation studies, and inclusive spectra measurements, should be codified into a comprehensive phenomenology; an example is the Reggeon calculus. This phenomenology should eventually be comprehended in terms of a fundamental theory of the strong interaction such as QCD. While such studies are in a nascent state, one can expect growing interest in the subject over the coming years. Tev II, Tev I, and ISABELLE are all important contributors. Meson spectroscopy at SLAC and AGS energies is also far from complete, and such work should be important in comparing properties of light quarks to heavy quarks.

In a more speculative vein, the search for fractionally-charged hadrons is in fact a central test of the confinement hypothesis. It is hard to conjecture an optimal facility for such a search.

Another area of interest lies in the possible production of quark-gluon plasma in high-energy heavy-ion collisions. This subject is new and at present being actively pursued. The capability of ISABELLE eventually to store heavy ions makes this an attractive prospect for the longer-range future.

C. Contributions of the Facilities to Physics Beyond the Standard Model

Any discussion which goes beyond the standard model must be regarded in a more tentative way than the discussion in the previous Subsection. The topical theoretical concepts (extended gauge groups, technicolor, supersymmetry, compositeness) which imply excursions from--or modifications of--the standard model lack coherence and internal consistency at present. They are subject to relatively frequent revision. A recurrent theme of these efforts is dissatisfaction with the conventional description of the forces associated with the Higgs sector. As we mentioned before, it is especially in this area that one can most confidently expect surprises. While the theoretical ideas are unreliable, they do provide prototypes for what might actually be present, are guideposts for experimental searches, and may be reasonable bases for comparing the possible contributions of future facilities.

The electroweak SU(2) X U(1) theory may not be correct. There may be no intermediate boson or there may be extra bosons, especially Z^o's. Proton-proton or proton-antiproton colliders such as ISABELLE and Tev I are excellent facilities for carrying out a search for such heavier Z^o's. The range of mass sensitivity runs, in the context of gauge theories, from present lower limits of 30-60 GeV to somewhere near 300 GeV for both Tev I and ISABELLE, with the high luminosity of ISABELLE a significant advantage. In the context of the original Fermi theory, the sensitivity extends to 900 GeV for the Tev I collider. Electron-proton collisions also hold great promise in providing an indirect but quite sensitive probe of heavier W's and Z's.

Alternatives to the standard picture typically predict a proliferation of new particles. The list includes heavier quarks, heavier leptons, charged Higgs bosons, leptoquarks, technihadrons, axions, and supersymmetric partners of conventional particles. Most (but not all) of these can in principle be found in electron-positron colliders such as SLC or CESR II provided the mass of such particles is less than 40 or 50 GeV (the LEP II project may eventually extend this number to 100 GeV). Some others, such as leptoquarks, may be better found in electron-proton colliders. Conjectured particles with masses in the 50-200 GeV range may be produced with proton-proton or proton-antiproton colliders. Production models for many of these particles--including some of mass 200-400 GeV--indicate that the cross-sections are high enough for them to be detected. Massive axial gluons, associated with spontaneous breakdown of a chiral SU(3) X SU(3) color group, might be detectable up to a mass scale of 400-600 GeV. Detection of any of these particles in the midst of a large background is difficult. The high luminosity here helps ISABELLE, while the lower background rate per event (owing to the typical sharp rise in cross-section with energy) helps Tev I.

It is possible that particles we today consider pointlike may be composite extended structures. This is at present a matter of serious theoretical consideration, extending to many of the particles we have discussed. The search for compositeness is a basic driving force toward higher energies. Most of the facilities (SLC, CESR II, Tev I, ISABELLE) can be expected to set strong limits on the scale of compositeness or to directly observe the effects. The sensitivity level for most of these facilities is in the several hundred GeV range.

D. Intangibles

The kind of analysis made in the preceding Subsections cannot be blindly trusted. History teaches that the course of scientific progress is tortuous, with discoveries of great significance coming from unexpected directions. For example, according to the classifications we have given, the best machine for discovery of the upsilon would have been an electron-positron collider; nevertheless, it was found in proton-nucleon collisions. One could hardly have anticipated at the time of construction of the AGS that discovery of CP violation would be one of its greatest contributions.

While by definition, we cannot talk in detail about surprises, the general capabilities of a given facility for providing surprises may be considered--and these are of great importance. Each facility has its own, often unique, capabilities. An evident capability of fixed target facilities is their high effective luminosity and concomitant diversity of approach. One measure of this is that, even from the point of view of exploratory "frontier" science (as opposed to programmatic studies), such facilities approach obsolescence only very slowly. The discovery of the J/psi at the AGS occurred 14 years after commissioning of the machine. At this time highly exploratory experiments are being performed at the oldest facilities, namely the SLAC linac and the AGS. One has no reason to expect less longevity of the Tev II program at Fermilab.

With regard to colliders, the oft-stated and well-proven argument on behalf of electron-positron colliders is "anything charged (with mass less than the beam energy) will be produced." With the next generation of electron-positron colliders, this can be generalized to "anything, charged or neutral, with electroweak interaction, will be produced." Furthermore, backgrounds tend to be relatively small, so that detection problems are minimized.

With the advent of QCD, a similar argument applies to the high energy proton-proton and proton-antiproton colliders: "anything colored will be produced." Here background is higher, but this is somewhat compensated by the higher energy and, in the case of ISABELLE, higher luminosity.

In all cases, it is self-evident that the exploratory capabilities of the colliders are very strong.

E. Summary: Contributions of the Hadron-Hadron Colliders to Physics

In this Subsection we abstract from the previous discussion those elements of the program to which hadron-hadron colliders make strong contributions.

Within the context of the standard model, even though electron-positron colliders such as SLC and CESR II play an especially important role, there are also important roles for the hadron colliders not only in studies of W and Z production dynamics, but also in studies of dilepton production and the properties of high transverse momentum jets. These studies are excellent tests of the strong interaction and should greatly contribute to our understanding of QCD. In addition, if the top quark has a mass less than about 100 GeV, Tev I and ISABELLE would have the opportunity of finding it.

Within the context of physics beyond the standard model, the hadron colliders have the capability of searching for additional weak gauge bosons in the mass range up to 300 GeV. Many other particles are conjectured according to current theoretical ideas. If these particles have masses in the multi-hundred GeV range the proton-proton and proton-antiproton colliders can effectively search for them. These machines should be excellent exploratory tools at an energy scale above that set by the standard-model Z^0.

Finally, there is the question of intangibles, i.e., the general capability of hadron colliders to produce surprises. The foremost consideration in this regard is their high energy. Even though the colliding protons are compound structures, the center-of-mass energies for collisions of their constituents are in excess of what will be attained with electron-positron colliders. As we have already mentioned, this energy region may be one rich in new phenomena.

F. General Comments

The ISABELLE and Tev I projects are in large measure complementary. If one considers very high energy constituent subprocesses (above 200-400 GeV), then Tev I has the advantage due to its higher energy. However, this capability is limited to processes which have relatively high cross sections. ISABELLE has the advantage in the energy region below 200-400 GeV. There the higher luminosity gives higher rates for all processes and allows greater sensitivity for searching for low cross section processes.

Tev I has one fully-developed interaction region and one partially-developed region. It is constrained by the fact that the same superconducting ring serves as both a storage ring and an accelerator (feeding the fixed target program). It also has to share its operating time with the Tev II program at Fermilab. The net result is a machine of somewhat limited scope but one which explores uniquely a high energy range and which proves out a technology that might be applied to super-high energy machines.

ISABELLE is a direct current machine with six interaction regions, completely dedicated to colliding beam physics. The fact that it is a direct current machine, while Tev I has a duty cycle of no more than 10^{-3}, implies that for the same instantaneous rate the luminosity can be 1000 times higher in ISABELLE than in Tev I. This factor of 1000 is also just the factor by which the intrinsic design luminosities of the two machines differ.

ISABELLE is unique in that no other high luminosity two-ring collider is contemplated. Indeed no other dedicated hadron colliding beam facility is now proposed. In the long run it has capabilities extending beyond the program of proton-proton collisions. Addition of an electron ring for electron-proton collisions is an attractive option, provided it is not redundant with an electron-proton initiative elsewhere such as the European HERA proposal. Likewise polarization of the stored proton beams may yield new insights into the physics of W and Z^0 production or other processes. Finally the introduction of heavy ions may open up a whole new area of investigation of properties of quark-gluon plasma--a state of matter believed to exist in the first 10^{-6} seconds of the big bang.

SECTION IV

TECHNICAL ASSESSMENT

The Technical Subgroup of the Subpanel, P. Reardon, W. Schnell, E. Knapp, A. Sessler, and consultants, J. Rees, R. Lundy, and C. Taylor visited Brookhaven on October 8 - 9, 1981. A Subgroup of P. Reardon and A. Sessler, with consultants J. Rees and R. Jameison, visited Fermilab on October 1 - 2, 1981.

Clearly, technical considerations of ISABELLE are germane to the deliberations of the Subpanel's interim report. Because the Tev I project is also aimed at high energy hadron colliding beam physics, it was felt necessary to examine both projects prior to November 1, 1981. Comments upon these two projects are presented in this Section.

A. Description of the ISABELLE Project

ISABELLE is a proton-proton collider designed for a maximum energy of 400 GeV x 400 GeV. Proton beams circulate in opposite directions in two separate rings each of 3,834 meters circumference. Superconducting dipole magnets with 5 tesla field are required to achieve this. Separate rings, crossing each other only where collisions are desired, permit the use of unbunched direct current beams. This eliminates a number of intensity-limiting effects and leads to a higher beam-beam limit than would obtain with bunched beams. As a result, intense beam currents can be stored and very high luminosities can be reached.

For ISABELLE, beam currents of 8 amps per ring are stacked at 29 GeV energy by multiple transfers from the AGS. These beams are then rebunched (at the third revolution harmonic), accelerated to the top energy and debunched again. Collisions are provided in six interaction areas, four of which will be initially equipped for experiments. The two beams cross each other at 11.2 mrad in low-beta insertions with $\beta = 7.5$ m vertically and 42.9 m horizontally. They will provide $2 \times 10^{32} cm^{-2} sec^{-1}$ nominal luminosity with as much as 60 m free space for the experiments. Considerably higher luminosities, at the cost of reduced free-space, can be reached by lowering the crossing angle and the beta functions in the interacting region. Maximum luminosities of about $5 \times 10^{33} cm^{-2} sec^{-1}$ appear to be achievable in this way.

B. Description of the Tev I Project

The Tev I project is a proton-antiproton collider which will provide a luminosity of about $10^{30} cm^{-2} sec^{-1}$ at an energy of 1000 GeV x 1000 GeV.

To achieve this goal, protons from one booster batch are to be accelerated, in 80 bunches, in the Fermilab main ring to 120 GeV. The proton bunches are then compacted by radiofrequency manipulation from 9 nsec to less than 1 nsec, and extracted and focused to a spot of 0.38 mm on a tungsten target. The antiprotons which are created are collected by a short focal length lithium lens and are then injected into a debuncher ring. This ring, which has a central momentum of 8.9 GeV/c, has an acceptance of ±2 percent in momentum and 20π mm-rad in each transverse dimension and thus can accept 1.25×10^{8} antiprotons from each booster batch. The antiprotons are debunched by a factor of 10 in the debuncher ring and then extracted and injected into an accumulator ring. In this ring, by means of stochastic cooling, the antiproton density in both transverse dimensions is increased by a factor of eight, while the longitudinal (energy) density is increased by a factor of 10^{4}; so that finally 6×10^{11} antiprotons are in a core of 1.1 MeV. Approximately 6×10^{10} antiprotons, which are accumulated in 1.3 hours, are taken from the accumulator, and transferred to the main ring, and accelerated to 150 GeV and then transferred to the Energy Saver. This sequence is repeated three times. The resulting proton-antiproton luminosity, with a total of three bunches in each beam and a β^* value of one meter, is 10^{30} cm^{-2} sec^{-1}.

C. Comments upon the ISABELLE Project

1. Management

In the last four months, since N. Samios was appointed BNL Deputy Director for High Energy Physics, there has been an improvement in the ISABELLE management structure. Several critical policy decisions relative to ISABELLE have been taken (adoption of one of the new cable magnets, the 1984 goal for test of first sextant, etc.) and a reorganization is underway which should improve internal communications and more sharply delineate lines of authority and responsibility. There is a recognized need for the appointment of a Project Director and a search (difficult under the present circumstances) is underway to find a senior individual to take over the project and accomplish the in-depth planning and scheduling, including the integration of the R&D effort into the construction program. A planning committee has been formed and all of the magnet activities have been integrated into a revamped magnet division. Reorganization of the cryogenic, rf, and vacuum systems effort is underway and the morale of the key individuals of the project is reasonably high now that an attractive magnet option has been chosen. Overall morale has, of course, been eroded by the uncertainties associated with continuation of the project, particularly in the light of recent further budget cuts.

2. Magnets

BNL has decided to set aside the previous magnet design (which featured "braid" with the preload supplied by differential thermal contraction of the iron yoke) and to adopt a new design incorporating Fermilab-style Rutherford cable preloaded by bolted-up split iron yokes. This provides an excellent basis for a magnet development and construction program which should lead to a satisfactory set of magnets for ISABELLE. The first two short prototype magnets (CM-1, CM-2) have demonstrated excellent maximum field capability, namely 5.85 tesla (at 3.7°K) with essentially no training, and indicate that the coil and yoke design provide the necessary mechanical properties for large aperture high field magnets. The design also addresses field quality, ramp-rate sensitivity, electrical insulation, problems of differential thermal motion and quench protection.

There is reason to expect that 15-foot long magnets will exhibit the same stability as the 5-foot models. However, since the causes of magnet instabilities (premature quenching, etc.) are not well understood, one cannot be sure of this design until a long magnet has been operated under forced-flow cooling conditions. BNL is putting appropriate emphasis on the achievement of this goal.

A preliminary plan for a magnet development program now exists which features the testing of materials and production techniques over the next several months while a production capability of one or more magnets per week and a matching magnet testing facility are developed. This program will involve the construction of approximately 35 magnets as part of the "learning curve" and lead into the production of at least 54 dipoles and 23 quadrupoles which would be installed in the existing tunnel for a sextant test. The early part of the "learning curve phase" will explore the cost effectiveness of various split yoke and support tube designs, the properties and control of the epoxy used to impregnate the fiberglass insulating tape, the performance of correction coil packages incorporated into full length dipoles and quadrupoles, the ability to protect magnets with internal diodes and quench-propagating heaters, and similar items, some of which are undoubtedly not yet foreseen. All of these questions must be answered as quickly as possible in order that production of identical units may commence which will allow a test of field reproducibility and magnet reliability.

The major goals of the magnet R&D program are outlined as follows:

a. Test the first long magnet, LM-1, by October 31, 1981.

b. Continue to produce long coils using iron yokes of LM-1 and LM-2. Test eight magnets by April 1982. This will establish reproducibility of training behavior and develop coil fabrication techniques.

c. By April 1982 several additional 5-foot models will have been tested, using the CM-1 and CM-4 iron yokes, to establish details of the end winding design for proper field quality.

d. By June 1982, one full-length dipole per week will be produced. This will continue until approximately 35 pre-production magnets have been tested.

e. By April 1984, approximately 100 magnets will have been built with 54 dipoles and 23 quadrupoles assembled into a sextant ready for test with beam.

f. By late 1984, these magnets will be cooled down and operated in the ISABELLE pulsed mode to provide an integrated systems test.

The latter portions of the production effort will provide a basis for more accurate labor and material costs for the magnets for the remaining eleven sextants.

This is an ambitious R&D schedule. With adequate financial resources and proper management this schedule can probably be met by the BNL group. Completion of this R&D program will provide an adequate test of magnet production capability and enable BNL to proceed with full-scale magnet production.

3. Cryogenics and Transfer Line

The overall cryogenic system for ISABELLE seems sound. There appears to be enough refrigeration capability at BNL for magnet testing. Problems experienced with prototype transfer lines, supplied by industry, seem to be due to poor design and workmanship. These can be solved by proper attention to inspection at the factory by BNL.

4. Other Accelerator Systems

The ISABELLE lattice appears to be a sound design. The arcs offer reasonable flexibility for a superconducting system, namely, the possibility of a few integers common tune variation in the two planes and of up to one or two integers vertical-to-horizontal tune difference. The matched insertions containing the collision points offer enough flexibility to cope with unforeseen beam dynamics situations. The possibility is foreseen to lower the collision-point beta-function and the crossing angle of the two beams (with some redesign and refitting) below the nominal design values in order to gain extra luminosity at a later stage of development or to reach design luminosity should there be difficulties in achieving the design beam current of 8 amps. This seems to present a valuable safety factor in the basic ISABELLE design.

The vacuum system for a storage ring with high-intensity coasting proton beams presents difficult design problems because of the need for very low pressure, efficient clearing of ionizaton electrons, and a high degree of cleanliness of the chamber walls to prevent pressure runaway by ion-induced desorption. The basic features of the ISABELLE vacuum system are taken over from the proven CERN-ISR design (e.g., in-situ bake-out and glow-discharge cleaning prior to installation), but additional problems arise from the need to insert the vacuum system in a superconducting magnet. The design of the ISABELLE vacuum system has reached a high degree of maturity and has undergone extensive and successful testing in the form of detailed prototypes. Production could therefore start at any time with full confidence.

The remaining accelerator systems have not received the same attention. Examples are injection, stacking, acceleration, bunch-to-bunch feedback (required to fill the ISABELLE circumference with bunches from successive AGS cycles at achievable flat-top field tolerance), beam diagnostics and beam dumping. For most of these systems only conceptual designs or preliminary models of critical components exist, and several basic design choices have yet to be made. The acceleration system (three 3rd-harmonic cavities per ring) is considered a problem because of the combination of the required high voltage and low permitted impedance presented to the beam. Model work on this problem and on the design of the stacking cavity is in progress. For most of the other systems detailed design work appears to be postponed to a later time in the construction schedule.

There appear to be no insurmountable problems involved in any of these systems and it does seem possible, and indeed justified under the present circumstances, to defer much of this work to a later date. It would seem urgent, however, to prepare a detailed plan and time schedule covering all the outstanding design and prototype work, from basic decisions to detailed engineering.

The stated initial luminosity goal of $2 \times 10^{32} cm^{-2} sec^{-1}$ with 8 amps stored in each beam appears to be realistic. For comparison, the ISR with an original goal of $L = 4 \times 10^{30} cm^{-2} sec^{-1}$ at circulating currents of 20 amps per beam has now reached $L = 1.2 \times 10^{32} cm^{-2} sec^{-1}$ with the help of a low beta insertion and stored currents up to 40 amps. Thus, later achievement by ISABELLE of luminosity up to $5 \times 10^{33} cm^{-2} sec^{-1}$ through improvements seems reasonable.

5. ISABELLE Construction Cost and Schedule

BNL is in the process of developing a new cost and schedule for the ISABELLE project in accordance with guidelines established by DOE. There will be a detailed cost and schedule available on November 1, 1981, as a part of a formal submittal to DOE of a new project baseline with a revised Schedule 44.

The Subpanel notes that the total of construction cost, R&D cost, pre-operating cost, and equipment cost (including detectors) from FY 1983 onwards would be about $500M (FY 1982 dollars). Although no details were presented, the Subpanel was concerned about the adequacy of the contingency allocated for magnet production in the interim cost estimate and believes that an additional $10-15M may be required.

6. Remarks on Accelerator Physics

The ISABELLE project will advance accelerator technology in two major ways. First, it will provide experience in this country with high luminosity proton-proton colliding beams. This experience has been acquired by CERN over the last decade, but in the United States there is no such experience. The second major contribution is experience with building and operating a superconducting accelerator. Such experience is being acquired at Fermilab, but the ISABELLE project extends this important expertise to a second laboratory and a wider group of people.

The ISABELLE project will also advance accelerator technology by providing experience with a different, wide-aperture, higher-field magnet than the Energy Saver magnet. The present shift in emphasis to testing of a complete sextant is an appropriate and important intermediate milestone. It properly focuses attention on a major integrated systems test. In our opinion, this would not be an essential R&D project for subsequent proton accelerator or storage ring facilities if ISABELLE were not to be completed.

D. Comments Upon the Tev I Project

1. Specific Remarks

The attainment by the Tev I project of luminosities of the order of $10^{30}\text{cm}^{-2}\text{sec}^{-1}$ usable for experiments depends on: a) the satisfactory performance of the Energy Saver ring as a storage ring; b) the success of the cooling systems in concentrating the antiproton beam to the necessary densities in phase space; and c) adequate antiproton production rates by the main ring and antiproton target system.

a) The Energy Saver ring will have a usable aperture of ± 15 mm over which the magnetic guide field is of sufficient quality for storage ring service. An orbit correction system is being built into the superconducting magnet ring which is designed to reduce residual orbit distortions to 1 millimeter or less. Given the expected transverse emittances at 150 GeV (the energy at which the beams are their largest) the transverse dimensions of the beams will be only a few millimeters at the worst place on the circumference. Taking orbit distortions and beam size together then, only about 30 percent of the aperture need be occupied by 95 percent of the beam particles at 150 GeV and correspondingly less at 1000 GeV. These figures appear quite safe.

The designs of the low-beta insertions for the interaction regions are not yet complete. Designs for interaction regions beta-functions of 1.0 meters in both horizontal and vertical planes are in hand (at least conceptually), and these values yield $L = 0.5 \times 10^{30}\text{cm}^{-2}\text{sec}^{-1}$ with one bunch. (At the SPS an insertion with $\beta_x \lesssim 2$ meters and $\beta_y \lesssim 1$ meter has already been tested with protons.) In Tev I, luminosities up to $5 \times 10^{30}\text{cm}^{-2}\text{sec}^{-1}$ should be attainable with multiple bunches in each beam.

The colliding beam strength parameter, ξ (approximately equal to the beam-beam tune shift $\Delta\nu$), is only 0.002 to reach a luminosity of $10^{30}\text{cm}^{-2}\text{sec}^{-1}$. For the higher values of luminosity of 3 or $6 \times 10^{30}\text{cm}^{-2}\text{sec}^{-1}$ the ξ value is 0.003. For electrons where there are quantum fluctuations in the radiation and damping, there is considerable experimental information which shows successful operation of storage rings if $\xi \lesssim 0.02$. For protons there is little knowledge. The ISR has an unbunched proton beam and in normal operation no beam-beam effect has been observed for $\xi = 0.001$. Tev I has bunched beams, and hence an additional degree of freedom in the motion. An experiment with a non-linear lens on the SPS suggests that $\xi \lesssim 0.003$ is acceptable. The CERN Antiproton Accumulator project can soon be expected to supply some information on colliding beams.

b) In order to reach the proposed luminosities, the cooling ring is required to produce a cooled "core" of antiprotons in the stacked beam which is about 10^4 times denser in energy space (antiprotons per unit of energy spread) and eight times denser in each transverse phase space (horizontal and vertical) than in the injected batch. The central portion of the core must contain at least 10^{11} antiprotons per MeV within horizontal and vertical emittances of 2.5×10^{-6} mm-rad. (Emittances are defined to contain 95 percent of the distribution.) The concentration of the beam in the three dimensions of phase space is to be carried out exclusively by stochastic cooling systems similar, but not identical, to those used in the CERN Antiproton Accumulator now successfully in operation.

The rate of delivery of antiprotons to the cooling ring is anticipated to be about 10^8 particles every 2 seconds which leads to a collection time of 4 hours for 6×10^{11} antiprotons stored in the cooling ring and a central density of 10^{11} antiprotons per MeV in the core. Transfers of antiprotons to the Energy Saver for colliding beam operation will remove only 8×10^{10} particles for each batch transferred. At most, three such batches would be transferred to constitute a fill of the Energy Saver, and that loss could be replenished in about 1.3 hours. Four hours would be required to obtain the final goal of $5 \times 10^{30} \mathrm{cm}^{-2} \mathrm{sec}^{-1}$. If beam lifetimes of the order of a day are achieved in the Energy Saver, such collection times will not significantly degrade the average luminosity, but significantly shorter beam lifetime would threaten performance. This is a major area of concern at CERN. With short proton bunches a lifetime of over 10 hours has been observed, but with the longer antiproton bunches available so far, the observed lifetime is only 1 hour.

The CERN Antiproton Accumulator ring was designed to accumulate 6×10^{11} antiprotons in 25 hours. After one year of operation it has achieved 1×10^{11} in about 30 hours. The Tev I design takes account of this CERN experience and overcomes the long accumulation time by means of: (1) higher production rate by means of higher energy protons; and (2) enhanced capture via the Lithium-lens, rf manipulations, and a buncher ring. Various longitudinal (radiofrequency) manipulations of the antiproton beam are required to introduce the particles into the cooling ring and to extract the central portion of the core from it. Although these procedures require precision, they are straightforward and do not appear to constitute a danger to the success of the project.

c) The antiproton production system uses a tungsten target and a Lithium-lens collection system. The antiproton yields are calculated from a phenomenological fit to the world's cross-section data which shows the yield into the momentum acceptance of the cooling system to be rather flat over the range of central antiproton momenta from 8 GeV/c to 13 GeV/c. With an incident proton spot size of $\sigma = 0.38$ mm and energy of 120 GeV, the yield of 8 GeV antiprotons into the downstream acceptance ($A_x = A_y = 20\pi \times 10^{-6}$ meter-rad and $\Delta p/p = \pm 2$ percent) is 5×10^{-5} antiprotons per proton. This spot size should permit targeting as many as 3×10^{12} protons per pulse without thermal destruction. Fermilab proposes to target only 2×10^{12} protons once every two seconds to produce the 10^{8} antiprotons required by the cooling system.

Fermilab already has a Lithium-lens of smaller aperture while the collaborators at Novosibirsk have built another lens which would meet the specifications of the present application. There is no experience yet with a Lithium-lens in a beam.

2. General Remarks

The Tev I project has certain risks associated with it such as beam manipulation by rf, stochastic cooling, beam-beam collision with bunched non-radiating beams and efficient collection of antiprotons. On the basis of experience in this country and knowledge being acquired at CERN on the Antiproton Accumulator ring, we believe that these risks are acceptable. Problems, if they arise, will often be outside the main ring enclosure and, hence, will delay colliding-beam physics, but not fixed target physics. The attainment of a luminosity of $10^{30}\,\mathrm{cm}^{-2}\mathrm{sec}^{-1}$ would appear to be a reasonable goal. We note that this achievement requires efficient antiproton collection, effective cooling of the antiprotons, good manipulation and transfer of the antiprotons and a sufficiently high beam-beam limit, but we believe that all of these requirements can be met.

The achievement of a luminosity greater than $10^{30}\,\mathrm{cm}^{-2}\mathrm{sec}^{-1}$ will, in our judgment, take some years of operational experience. A number of improvements seem possible. Thus, an ultimate goal of $L \simeq 10^{31}\,\mathrm{cm}^{-2}\mathrm{sec}^{-1}$ appears reasonable to us.

The team assembled by Fermilab is competent and strongly motivated. A major reorganization was accomplished by integrating the accelerator and Energy Saver effort with the Tev I effort. We are impressed with the group and feel that it is basically quite adequate to be given the considerable responsibility of the Tev I project. However, some additional help may be required in the area of the accumulator and the damping ring.

The Subpanel notes that the total of construction cost, R&D cost, pre-operation cost, and equipment cost (including detectors) from FY 1983 onward would be about $70M (FY 1982 dollars). Detailed schedule and costs of the project are expected to be available by January 1982. The Subpanel believes that an additional $15M may be required to complete Tev I. With the annual funding constraints expected, the start up may be delayed until the end of 1985.

3. Remarks on Accelerator Physics

The Tev I project will advance accelerator technology in a number of ways. Most importantly, we shall gain experience in this country with stochastic cooling of antiprotons and with the operation of a high luminosity proton-antiproton storage ring. Experience with the operation of a superconducting ring is vitally important, but will presumably be gained with the Energy Saver project. In the future, stochastic cooling should become a readily usable tool of accelerator physics and, as such, will have many applications. Foreseeable applications are to a multi-TeV proton-antiproton project or, more immediately, to cooling in the Energy Saver ring itself. The latter could result in improved performance of the Tev I project. Thus, experience with an operational cooling system is a most valuable attribute to the Tev I project.

Future hadron colliders may have bunched beams and cooled antiproton beams, hence, the Tev I project including the associated detector developments will give experience which is applicable to future colliders.

APPENDIX A

CHARGE
SUBPANEL ON LONG RANGE PLANNING
FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM

Within the context of changing worldwide high energy physics activities and opportunities, review the status and prospects of the U.S. program (DOE &NSF) taking into consideration all aspects of the program.

Develop a strategy and long-range plan for the U.S. program over the next decade under the following funding constraints:

1. The DOE/OMB Agreement of 1978 which is equivalent to a DOE level of $423M in FY 1982 dollars. The NSF FY 1979 level of $25M for elementary particle physics is equivalent to $35M in FY 1982 dollars.
2. A level 10 percent lower.
3. A level 10 percent higher.

Within each of the above scenarios assess the prospects and sufficiency of the strategy and long-range plan to maintain a forefront U.S. high energy physics program.

In view of the pivotal role which the ISABELLE project has on long range planning please submit an interim report which evaluates the priority of ISABELLE in each of the three constraint cases, taking into consideration the physics potential of the facility, the funding and schedule required to complete construction, and competing needs and opportunities.

The interim report should be submitted by November 1, 1981, and the report should be completed by January 1, 1982.

APPENDIX B

Membership and Consultants

Subpanel on Long Range Planning
for the U.S. High Energy Physics Program

Membership

G. Trilling, Chairman - U. California, Berkeley/LBL
B. Barish - California Inst. Tech.
K. Berkelman - Cornell U.
J. Bjorken - Fermi Nat. Acc. Lab.
J. Cronin - U. Chicago
H. Feshbach - Mass. Inst. Tech.
V. Fitch - Princeton U.
E. Knapp - Los Alamos Nat. Lab.
R. Palmer - Brookhaven National Laboratory
M. Perl - Stanford Linear Acc. Center
H. Politzer - California Inst. Tech.
P. Reardon - Princeton Plasma Physics Lab.
J. Rosner - U. Minnesota
W. Schnell - CERN, Geneva
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R. Taylor - Lawrence Berkeley Laboratory

THE U. S. DEPARTMENT OF ENERGY HIGH ENERGY PHYSICS ADVISORY PANEL REPORT OF THE 1980 SUBPANEL ON REVIEW AND PLANNING FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM

JULY 1980

STANFORD UNIVERSITY

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July 15, 1980

Dr. Edward A. Frieman
Director of Energy Research
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Dear Ed:

I am forwarding to you the report of the 1980 HEPAP Subpanel which was formed in response to the Charge to develop a general strategy and long range plan for the U.S. High Energy Physics program over the next decade. In particular, the Subpanel was asked to assess the program balance between research, equipment, and construction over the period from FY 1982 to FY 1987 and to make specific recommendations for FY 1982. Two funding constraints were given as guidance to the Subpanel: the DOE/OMB Long Range Plan of 1978 - i.e., $325M in FY 1979 dollars for DOE plus NSF; and a funding level that is 10 to 15 percent higher. In addition, the Subpanel was asked to identify the physics opportunities that would have to be deferred, or that would be lost, under those constraints.

In addressing this broad and difficult Charge, the Subpanel worked both hard and effectively in the time available to it, and I greatly appreciate their efforts. HEPAP discussed the Subpanel report extensively at its June 30-July 1, 1980 meeting at DOE Headquarters in Germantown, Maryland. We endorse its general conclusions and recommendations as described below. In this transmittal letter I will convey HEPAP's own set of priorities on the main recommendations of the report, where they have not been expressed by the Subpanel. I also wish to express HEPAP's serious concerns about the future of the national High Energy Physics program in view of the gap between the current funding levels and the guidance given to the Subpanel.

High Energy Physics is in the midst of very exciting developments, both experimental and theoretical, that radically altered our views of the basic forces and elementary constituents of nature during the 1970's. Many of the key discoveries during the past decade were made in the United States. We are now entering the 1980's with a powerful and beautiful new conceptual framework being constructed which unifies three seemingly very different forces with one another: the electromagnetic forces, the weak forces of radioactivity, and the strong nuclear forces. At the same time many basic questions about the nature of these forces and the elementary constituents which they affect still remain unanswered. We see the possibility of confirming or denying our emerging picture of nature on the sub-nuclear frontier with decisive experiments using the new detectors and accelerators presently starting initial operation as well as the facilities now under construction. Other facilities currently being designed and analyzed would permit further crucial tests of the validity of this emerging picture. HEPAP recognizes that it is essential to formulate long range as well as short range plans in a field of such fundamental importance as High Energy Physics that requires large and expensive facilities as well as talented scientists in order to achieve its progress.

The national High Energy Physics program of the United States is heavily committed at this time to two major construction projects based on superconducting magnet technology at Fermilab and at Brookhaven. These are bold and exciting ventures that HEPAP endorsed in its earlier facilities reports. We again emphasize our strong support for these projects although they are turning out to be both technically more arduous and more expensive than previously anticipated. Though great strides have been made with the technology of superconducting magnets, some hurdles in this respect remain before both construction projects can be completed. These hurdles are described in the Subpanel report which notes that Fermilab, with its earlier commitment to substantial R&D, is further along toward its goal. When they are operating as proposed, the Saver/Tevatron I/Tevatron II at Fermilab and ISABELLE at Brookhaven will permit major advances in the research frontiers of elementary particle physics. We believe that this complex of new construction projects should proceed at this time with highest priority. The necessary R&D funds to assure their success must be provided during the coming year. At the same time we recognize that there is a great need to provide for high utilization of the forefront accelerator facilities in the ongoing research program--i.e., the Fermilab 400 GeV accelerator, the newly-commissioned PEP storage ring, the CESR facility at Cornell. As part of this recommendation, increased support is required for user groups to exploit these facilities for physics. The third component of a balanced research program--the detector and accelerator R&D that are essential for further progress--also requires significant support as emphasized by the Subpanel report. The technologies of very high charge-density bunches with low emittance in linear accelerators, very high-field superconducting magnets, and superconducting rf cavities were singled out by the Subpanel as being of great importance.

For the present we can meet the above goals of a healthy and balanced research program at the funding level of the 1978 DOE/OMB guidance. However, such a funding constraint still means the loss of very good science with existing facilities supported significantly below their potentials. In particular, at this level it is necessary to operate the currently interesting, important, and productive lower-energy fixed target programs at the Brookhaven AGS and SLAC linac with lower priority, thereby providing fewer opportunities for physics for considerable numbers of University and Laboratory-based user groups. This level of funding will also require deferral of other new construction initiatives. Specifically, the SLC electron-positron linear collider project has been proposed by SLAC, and other e^+e^- and ep colliders have been discussed. We would anticipate from projects such as these a very rich output of physics as well as advances in accelerator technology.

It is important to recognize that with only a modest (15 percent) increase in funding above the DOE/OMB guidance we could achieve our major goals of: timely progress toward completing current construction projects; major improvements in the level and efficiency of facilities operations, and correspondingly of valuable physics output from the ongoing programs; a strong program of advanced accelerator R&D. Simultaneously, an early new construction initiative such as described above, could be accommodated. This program would indeed ensure a broad-based, preeminent U.S. national program, both exciting in its own right and complementary in scope with Western Europe's very strong and advancing program. A U.S. program of such vigor remains our immediate and long range goal. As your Advisory Panel, charged with the responsibility of helping define a strategy and long range plan for a preeminent U.S. national High Energy Physics program, we can recommend no less.

The painful reality we must recognize and confront, however, is this: the High Energy Physics operating budgets for FY 1980 and, as currently envisaged for FY 1981, are well below the long range DOE/OMB plan of 1978. The best current estimate is that by the end of FY 1981 the accumulated shortfall since the initial implementation of this plan in FY 1979 will be about $45M. Budget stringency during this period has severely limited the R&D program underpinning the ongoing construction, and has also reduced the level of utilization of existing facilities to the point of causing a serious loss of physics as well as great inefficiencies and difficulties for the research groups. This loss will continue unless the funding is restored at least to the level of the DOE/OMB 1978 plan. Continuation of the current funding restrictions may force substantial reduction in the scope of the U.S. High Energy Physics program. If this occurs, the U.S. program will inevitably lose its eminence, due both to inadequate opportunities for U.S. scientists as well as to our inability to compete with Western Europe. We then will face the prospect of the U.S. High Energy Physics program being less and less competitive to share in making the major advances and discoveries on the sub-nuclear frontiers.

We hope that this will not be the fate of a U.S. program that has so distinguished a record of major accomplishments in the study of nature's most basic processes and constituents; a field that has flourished here for close to 50 years since its birth at the Lawrence Berkeley Laboratory under the initial leadership of E. O. Lawrence; and a field that has contributed so importantly to advancing the technology base of the U.S. In particular, pioneering advances in superconducting magnet and cryogenic technology, spurred by the Fermilab Energy Saver/Tevatron project and the ISABELLE project at Brookhaven, have important practical applications. This technology, which is so vital for further advances in the high energy frontiers, is also required for progress in the development of fusion energy and magnetohydrodynamics, and will contribute to new techniques for energy conservation.

We expect that another review of the general strategy and plans for the U.S. High Energy Physics program may very well be desired a year from now. This summer proved to be an especially difficult time to formulate long range plans; hence, it was almost inevitable for the Subpanel report to focus heavily on problems, moods, and opportunities of the ongoing program. A year from now, however, there may be a considerable clarification of uncertainties that weighed so heavily on the deliberations of this year's Subpanel. For one thing, the current funding projections were very uncertain. Further clarification of the financial plans for High Energy Physics can be hoped for following completion of Congressional action on the FY 1981 budget and after discussion of the long range program plans between the Congress and DOE/OMB subsequent to study of the impending report of the General Accounting Office. In addition and most importantly, a year from now we may expect to have higher confidence in clearing the technical hurdles in our current construction programs based on superconducting magnet technology, as well as more definite information on Western Europe's plans for their High Energy Physics program. It may then be timely to accomplish an updated review of our plans and strategy for ensuring continued U.S. leadership in the quest for an understanding of nature's fundamental laws.

Sincerely yours,

Sid

Sidney D. Drell
Chairman, HEPAP

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June 24, 1980

Professor S. Drell
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Dear Sid,

Enclosed is the Report of the 1980 Subpanel on Review and Planning for the U.S. High Energy Physics program, which met at the National Academy of Sciences Study Center, Woods Hole, Massachusetts, from June 1-7, 1980.

Sincerely,

Sam Treiman

Sam Treiman

SUMMARY

High energy physics addresses the fundamental structure of matter, a central theme of science. The field is in a state of high accomplishment and promise. Striking experimental, technological, and theoretical developments over the past decade have combined to transform our understanding of the basic constituents of matter and of the forces that govern their interactions. The picture of protons and neutrons as composites built up out of more basic entities, the quarks, has been strengthened and elaborated. The strong forces, responsible among other things for binding of nuclei, and the weak forces, responsible for radioactivity, have come to be understood at a deeper level corresponding to the understanding of electromagnetism, pioneered by Maxwell in the 19th century, and of gravitation, pioneered by Einstein in the early years of this century. The weak and electromagnetic forces, and perhaps also the strong ones, now all appear to be unified in a common theoretical framework of great breadth and elegance. These theoretical advances have enriched the traditional connections between particle physics and other disciplines--nuclear physics, condensed matter physics, pure mathematics, the physics of the big bang universe, etc. The technological advances have similarly spread to other areas of science and technology. The cultural and technological attraction of high energy physics is reflected in the growing level of research activity and investment abroad, particularly in Western Europe and the USSR but also increasingly in Japan and the Peoples Republic of China.

The accomplishments in high energy physics spring from a variety of research approaches and experimental tools. At the heart of the effort are the high energy accelerators--the descendants of Ernest Lawrence's cyclotron. In the U.S. these are now centered at Brookhaven National Laboratory (BNL), the Stanford Linear Accelerator Center (SLAC), Fermi National Accelerator Laboratory (Fermilab), and the Newman Laboratory of Nuclear Studies at Cornell University. They serve the research needs not only of the high energy physicists at these centers but also of the wider community at the Universities. The current array of proton and electron accelerators in the U.S. constitutes a powerful and diversified set of research tools; and major new proton and antiproton accelerator projects underway at Fermilab and Brookhaven will extend our reach to higher domains of energy and surely to new discoveries.

The U.S. has long occupied the position of world leader in high energy physics, thanks to our national tradition of technological ingenuity, diversity, and generous public support. However, that leadership is now coming to be shared with Western Europe; and amidst all its high scientific achievements and promise, the U.S. program finds itself confronted with a number of present difficulties and future challenges and choices.

(i) The level of financial support in Western Europe now substantially exceeds that in the U.S. and it is necessary for the U.S. to rely increasingly on special effort and ingenuity to keep our program at least well represented at the major forefronts. Recent scientific developments point to the great promise for the coming decade of very high energy electron-positron and electron-proton colliders. Ambitious and costly projects along these lines are presently under serious discussion in Western Europe. While it no longer seems financially possible for the U.S. to proceed in parallel by the scaling up of conventional technology, there is scope for more financially modest projects based on inventive new technology. Several attractive possibilities are under discussion in the community and were reviewed by the Subpanel.

(ii) In the current program, financial stringencies are imposing increasingly severe limitations on the exploitation of existing facilities, hence limitations on the opportunties available to the talented community of high energy physicists at the national laboratories and universities to mount experiments and follow out important scientific leads.

(iii) The new Fermilab and Brookhaven projects, which are based on pioneering superconducting technology, are turning out to be more arduous than originally anticipated. This is a serious challenge in its own right. It also impacts on the resources available to other elements of the national program.

Conclusions

The Subpanel was charged, among other things, to address the future directions of the national program: (a) on the basis of a constant dollar support level corresponding to the current financial plan; (b) on the basis of a 10-15% increase in the level of support. Since the situation is complex our evaluations and recommendations have to be read with care, in the context of all the supporting commentary. The following is a rough and compact summary of the main points.

1. General

(a) Utilization of the forefront accelerator facilities in the ongoing research program should be intensified, to exploit for physics the investments already made, and university groups should be given increased support.

(b) The new superconducting projects, the Energy Saver/Tevatron at Fermilab and ISABELLE at Brookhaven, must proceed with all deliberate speed. It is the responsibility of the entire high energy physics community to help in surmounting the difficulties now being experienced.

(c) Increasing support should be devoted to detector and accelerator R&D.

2A. Constant Level of Support

(d) The financial stringencies imposed by the present level of support will have to be accommodated by carefully planned reductions in the programs at the lower energy facilities.

(e) New construction initiatives will have to be foregone, at least for the nearer future until the time scales associated with the Fermilab and Brookhaven superconducting projects can be better assessed. Nevertheless, intensive R&D looking to the farther future should begin now.

2B. Increased Support Level

A modest (15%) increase in support, beginning in FY 1982, would make possible major improvements in the level of utilization and physics output for the ongoing programs. Moreover, it would provide the possibility of an early new construction initiative which addresses promising new areas of research. Specifically, the future U.S. program would be greatly strengthened by an electron-positron collider operating in the energy region between about 30 GeV and 100 GeV, where a rich output of physics is anticipated. Similarly, a facility designed to study high energy electron-proton collisions promises exciting physics opportunities. It may be that both the above goals can be met with a combined facility.

The Subpanel reviewed a number of attractive possibilities now under discussion in the community, in particular, the electron-positron linear collider SLC proposed for construction by SLAC. The Subpanel recommends that no action on this proposal be taken now but it recommends that it be considered, along with other emerging alternatives, by a similar panel convened within a year or at most two. At an increased level of support, we anticipate that the time would then be right for positive action on a new facility.

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SECTION I

INTRODUCTION

A High Energy Physics Advisory Panel (HEPAP) Subpanel was established in the spring of 1980 to address the Charge reproduced here in Appendix A. The Subpanel met at Woods Hole, Massachusetts, June 1-7, 1980. A list of the Subpanel members, consultants, and other participants at Woods Hole is provided in Appendix B. In preparation for its final deliberations the Subpanel carried out site visits to Fermi National Accelerator Laboratory (Fermilab), May 1-2; Stanford Linear Accelerator Center (SLAC), May 9-10; and Brookhaven National Laboratory (BNL), May 16-17. The Subpanel also heard presentations from Lawrence Berkeley Laboratory (LBL) at the SLAC site visit, and from Argonne National Laboratory (ANL) and the Newman Laboratory of Nuclear Studies (Cornell) at the BNL site visit. The input at the site visits was augmented by written responses to questions put in advance to the Laboratory Directorates concerning their current programs, future plans, and general opinions about the direction of the national high energy program. In addition, the Subpanel solicited the views of the entire high energy community, through a call for written statements from individuals and users groups and through wide-ranging discussions during the site visits. The responses were numerous and well thought-out and have figured into the deliberations of the Subpanel in an important way.

The 1980 HEPAP Subpanel is the fourth in a series of Subpanels formed from time to time to review the status of the national program in High Energy Physics and to address long-range plans. The earlier Subpanels (1974, 1975, 1977) were concerned chiefly with the issue of new research facilities required to advance the vitality of the program after a substantial hiatus in major construction projects. Several projects addressed by these Subpanels have reached completion or are now underway. In the meantime, numerous scientific and technological developments have opened up promising opportunities for further progress in the coming decade. It is timely, therefore, to review our present status with respect to facilities and to consider what new research tools are needed over the coming years for orderly exploitation of these opportunities. Beyond the matter of facilities, however, it seems especially appropriate just now to also address more general issues concerning the structure of the national program in High Energy Physics. This is so because an unusual number of problems, promising scientific prospects, and critical choices presently confront the U.S. high energy physics community.

The situation is surveyed briefly in the remaining paragraphs of this Section. More detailed discussion and specific recommendations appear in later Sections.

The accelerating pace of experimental discovery and theoretical development over the past decade have brought particle physics to a state of high promise and have influenced the pattern of facility construction addressed by the earlier Subpanels. The e^+e^- collider (CESR), which came into operation at Cornell in the fall of 1979, has turned out to be remarkably well-placed in energy to pursue the physics of the new "bottom" quark discovered somewhat earlier at Fermilab. The e^+e^- collider PEP is just now coming into operation at SLAC in a higher energy region appropriate for the physics of quark and gluon jets and other fundamental issues. The Energy Saver/Tevatron facilities under construction or planned at Fermilab are designed to provide a window on ultra high energies (the $p\bar{p}$ collider at 2 Tev in the center of mass) and to pursue varied physical phenomena induced by the high energy secondary beams of the fixed target Tevatron program. The pp collider ISABELLE under construction at BNL is designed to combine high luminosity and high energy (800 GeV in the center of mass) for study of hadron collisions.

The fixed target physics of the Tevatron project, when it comes into operation, will upgrade the 400 GeV program that presently constitutes the major high energy element of the national effort. The ongoing programs at the BNL Alternating Gradient Synchrotron (AGS), the SLAC electron-positron storage ring (SPEAR), and the SLAC linac carry much of the remaining current effort at lower energies. These latter facilities represent a valuable national investment already in place, and they cover regions of particle physics in which much important work remains to be done. Moreover, the SLAC linac serves as injector to the Positron-Electron Storage Ring (PEP), and the AGS is slated to inject protons into ISABELLE. The national program in High Energy Physics also involves experimental and research and development (R&D) activities not associated with high energy accelerators: e.g., cosmic ray experiments, searches for proton decay, neutrino experiments based on nuclear reactors, weak interaction studies at Los Alamos Scientific Laboratory (LASL) based on the Los Alamos Meson Physics Facility (LAMPF), quark searches, etc.

At all major levels -- experimental, technological, and theoretical -- the U.S. community has played a leadership role over the past several decades in the expanding science of particle physics. The facility construction projects undertaken in the 1970's were designed to sustain a vigorous U.S. position for the coming years, following a period of decline in real dollar funding and the phasing out of a number of lower energy accelerator facilities. The current level of funding for the national program is nominally governed by the DOE/OMB Long Range Plan of 1978. This is supposed to correspond to a steady annual funding level (DOE plus NSF) of $325M in FY 1979 dollars. It represents the lowest of several alternative levels of support which the 1977 Subpanel was charged to investigate for future planning. That Subpanel regarded the ongoing program and the ISABELLE and Energy Saver/Tevatron projects as workable on a "best effort" basis at this level, but it foresaw the possibility of strains developing.

Several problems have indeed arisen. For one thing, inflation in power and certain other technological costs has not been fully allowed for in the conversion to real dollar budgets, so that a deficit has begun to develop with respect to the DOE/OMB Plan. This has entailed a decrease in the utilization of existing facilities and has led to delay and in some cases abandonment of promising experiments. Moreover, the new superconducting technology involved in the ISABELLE and Tevatron projects is turning out to be more arduous than was anticipated in the reports of the earlier Subpanels. This is a challenge in its own right but it also impacts on the resources available to the ongoing program. Yet, for the longer run, the superconducting effort contributes to an important developing technology with potential applications outside of high energy physics; and for the physics program it provides an avenue for mitigating the power costs of accelerator operations while reaching higher fields and energies.

Another issue that increasingly confronts the U.S. community is the growing vigor of High Energy Physics activities abroad, particularly in Western Europe. The e^+e^- collider PEP has its counterpart in the PETRA facility at DESY in West Germany; the 400 GeV program at Fermilab is paralleled by a corresponding program at the SPS at CERN in Geneva; the pp collider ISR, at CERN, is without counterpart in the U.S.; and the overall level of funding in Western Europe substantially exceeds that supporting the U.S. program. Moreover, there is an increasing level of activity underway and planned in Japan, the Peoples Republic of China, and the U.S.S.R.

At CERN, a $\bar{p}p$ collider facility (540 GeV in the center-of-mass) is now under construction and is expected to come into operation well before the 2 Tev $\bar{p}p$ collider at Fermilab. Counterparts to the high luminosity ISABELLE facility and the fixed target Tevatron program are not presently envisaged for Western Europe. On the other hand, plans for a very high energy e^+e^- collider LEP at CERN (100 GeV in the center of mass in the earliest stage) and for an ep and e^+e^- collider HERA at DESY are now under serious discussion in Europe.

These developments abroad attest to the scientific attraction and importance of High Energy Physics. From the U.S. perspective, they have to be recognized as the inevitable limits which are coming into place on our long established predominance in High Energy Physics. At the same time, they provide welcome opportunities for increased scientific progress on the international scale, through collaboration, allocation of tasks, and friendly competition and cross checking.

As is reflected in the range of accelerator types described above, progress in particle physics calls for variety in tools and approaches. New facility projects are motivated from time-to-time by ongoing developments in the physics itself and in technology. Although it may no longer be feasible for the U.S. and Europe to pursue every new research opportunity in parallel, the sustained vigor of the U.S. program requires that we be at least well represented in the most promising lines of research. In present planning for the coming decade, the U.S. program stakes out the highest energies for the physics of hadron collisions, via the ISABELLE and Tevatron projects. Western Europe will probably emphasize the highest energies for e^+e^- and ep colliders, via the very ambitious and costly LEP and HERA proposals. There are fundamental scientific issues at stake in every one of these areas. In particular, recent advances in our understanding of the weak interactions strongly indicate that these interactions are mediated by gauge bosons, the Z^o and $W^{\pm}$ particles, with masses somewhat below 100 GeV. The several lines of approach bear on various aspects of these gauge particles: their existence, their production and decay characteristics, and the open possibility that there may be more than one class of such objects.

The e^+e^- collider approach is especially suitable for study of the expected rich variety of Z^o decay processes, including decay channels involving other anticipated (and perhaps unanticipated) objects such as "Higgs" particles, new quark and lepton types, etc. Similarly, the study of high energy ep collisions can extend the search for gauge boson effects to higher mass regions and it opens up new windows bearing on other aspects of the weak interactions. Although the European decisions regarding LEP and HERA have not yet been taken, the U.S. community, in planning for its own program in the coming decade, cannot fail to take into account the possibility of positive European decisions and vigorous implementation.

Several possible new projects, in varying stages of definition, have been proposed for the U.S. program and have been brought to the Subpanel for consideration.

(1) The Stanford Linear Collider (SLC) proposal for an e^+e^- collider at 100 GeV in the center of mass is based on ingenious new technology with promise for later relevance to ultra-high energy devices involving colliding linac beams. The project has provision for one interaction region. Compared to LEP, the SLC project is relatively inexpensive.

(2) The e^+e^- ring contemplated by Cornell at 100 GeV center-of-mass has four interaction regions and involves superconducting rf technology. Again, by the standards of LEP the Cornell project is relatively inexpensive, though much more costly than SLC.

(3) A possible ep facility, being explored by a Canadian consortium and independently by a U.S. group at Nevis Laboratory, Columbia University, was described to the Subpanel. It would involve a 10 GeV electron ring, used in conjunction with a high energy proton beam at Fermilab or BNL.

As with the Tevatron and ISABELLE, either of the above e^+e^- facilities would involve the challenge and opportunities of new technology. For the longer term it is clear that scientific progress rests on the development of advanced technologies, designed to extend the energy, intensity, and flexibility of accelerators and the precision and range of detectors and instrumentation. Much of the R&D effort in the current U.S. program is properly aimed at the short-term goals of existing or imminent projects. However, it seems increasingly important to upgrade the effort devoted to goals of longer range. This issue is addressed in more detail later in the report.

The above discussion lays out some of the major issues that have confronted the Subpanel. High Energy Physics, in its intrinsic scientific dimensions, has accomplished major advances over the past decade. The ongoing program, the newly completed research facilities, and the projects underway, constitute a set of promising ingredients for further advances. On the other hand, budgetary constraints are exacting a serious price in the utilization of existing facilities and pose the risk of foreclosing the timely development of new facilities of major importance. This occurs in the face of increasing research investment and activity abroad.

Finally, an issue that is interwoven among all the others and that was the subject of much discussion by the Subpanel concerns the most important resource of high energy physics research, namely, the people who practice this science. The reduction in the number of accelerator laboratories that has taken place in the past decade and the reduction in utilization of the remaining facilities have combined to limit the opportunities for small groups of people to exercise independence and to mount and carry out experiments in a timely fashion. Moreover, the physics objectives themselves seem increasingly to demand large detectors, large collaborations, and long time scales. These trends are not peculiar to the U.S. program. On the shorter-term view one may have to accept the realities of the situation, in the interest of efficiency and scientific output. For the longer run, however, both efficiency and output depend ultimately on the skills and ingenuity of individual people, qualities that are enhanced by practice and independence. In our long-term planning, provisions have to be made to preserve independence and variety of opportunities, to engage fully the talented pool of high energy physicists at the universities, and to attract new, young talent. How these considerations are to enter into concrete decisions about the overall program, and with what weight, is a very complex problem.

SECTION II

STATUS OF THE FIELD

Introduction

During the decade of the 1970's, there has been remarkable progress in our understanding of the subnuclear world. Of the four basic kinds of forces, at best only two -- electromagnetism and gravitation -- could in 1970 be described in highly fundamental terms. The other two forces -- strong and weak -- were understood only in relatively incomplete and descriptive terms. The high energy behavior of the weak force could not be predicted, although enough was known to give hints of landmark energies at which new kinds of phenomena could be expected to occur. A great deal of information on the properties of the strong force provided by 1970 a body of organizing principles but no fundamental theory. Experiments were just beginning to point the way toward a description of the nucleon in terms of quark constituents, with some promise of simplicity of description at short distances.

In 1980 the situation is quite different. There exist candidate theories for both strong and weak interactions, each formulated at a level as basic as the formulations of electrodynamics and gravitation. The theory of the weak interaction is quantitative and predictive at very high energies as well as at low energies, and has enjoyed a remarkable degree of experimental success. The theory of strong interactions has likewise been successful in accounting for a broad range of strong interaction phenomena.

This revolution in our viewpoint on strong and weak interactions has also been accompanied by much greater confidence in the picture of nucleons as composites of point-like quarks. This picture was very much advanced by the landmark discovery at BNL and SLAC of the J/psi particle in 1974 and its subsequent description in terms of charmed-quark constituents.

The field in 1980 has progressed much further than anyone could have dared to anticipate in 1970, and a central feature of the program of the 1980's will be either to firmly establish the validity of the standard picture or show it to be deficient. If the present theoretical optimism is confirmed by the experimental program of the 1980's, we will have largely accomplished an advance in our understanding of the basic forces of nature comparable to the establishment of the theories of electrodynamics and gravitation.

The Standard Picture

The experimental and theoretical developments of the 1970's have led to a definite theoretical framework. In this standard picture the number and kind of building blocks (six kinds of quarks, three kinds of charged leptons, and three kinds of neutrinos) are specified, as well as the basic forces between them -- electromagnetic, strong, and weak.

The theory of quantum electrodynamics (QED) has been well developed for some time. The predictions of the theory have been tested ever more incisively by experiments at e^+e^- storage rings, so that it is known to be accurate down to a distance scale of order 10^{-16} cm, about one thousandth of the size of a proton.

The strong force acts between quarks and is described by a theory called quantum chromodynamics (QCD). QCD is structurally very similar to quantum electrodynamics (QED). Just as QED implies the existence of photons, QCD implies the existence of similar quanta called gluons. Gluons (like quarks) are only seen indirectly. Recently, indirect evidence for their existence has emerged from experiments at the e^+e^- storage ring PETRA. This will be a major subject of investigation for the PEP storage ring now being commissioned. Earlier evidence for QCD emerged from the lepton scattering experiments carried out at SLAC, Fermilab, and CERN during the 1970's. Experiments on production of lepton pairs at Fermilab and elsewhere have also provided evidence supportive of QCD.

The weak force, which is responsible for radioactivity, is described by a theory similar in many respects to QCD and QED. However, the analogs of photons and gluons, the intermediate bosons $W^\pm$ and Z^o, are expected to have large rest masses, of order 100 times the mass of the proton. The theory unifies to some extent the electromagnetic and weak forces at these large energy scales.

The rapid development of this theory in the 1970's occurred as a result of major theoretical breakthroughs and a large body of precision experiments at BNL, Fermilab, and CERN using muon and neutrino beams. The discovery of the J/psi at SLAC and BNL in 1974, and its interpretation in terms of the fourth, or charmed quark, was another important element. A beautiful and delicate experiment at the SLAC linac using polarized electrons also provided important confirmation. By now there is a remarkably consistent and accurate body of data in support of the standard electroweak picture.

The structural similarity of the theories of strong and electroweak forces has suggested a further unification. The simplest such extension involves extrapolation of present theory by 13 orders of magnitude to energies of order 10^{15} GeV, at which point the synthesis of strong and electroweak forces becomes manifest. While the energies required to directly explore this domain are inaccessible, there are observable low energy effects which do occur. In particular, the proton is predicted to be unstable, with a lifetime of the order of $10^{29} - 10^{33}$ years. Experiments with sensitivity in this range are already underway.

Despite these beautiful concepts and encouraging experiments, there are aspects of the electroweak theory which suggest it is incomplete. The most important aspect has to do with the masses of the $W^\pm$ and Z^o and of the quark and lepton building blocks. A general theoretical framework which describes the origin of these masses does exist, although there are differing versions of the details. In any event, it appears inevitable in such frameworks that there exists at least one extra particle (the "Higgs boson") with properties which make it very difficult to detect. The expected mass range is anywhere from a few GeV to 100 GeV.

The actual pattern of observed quark and lepton masses remains totally enigmatic. The understanding of this pattern is one of the greatest challenges for the theorist. Neutrinos may also have mass, a possibility that has been recently revived. Novel experimental techniques on a wide range of energy scales are relevant to the search for effects due to neutrino mass. The AGS, for example, is well suited to undertake these searches.

The number of quarks and leptons is also not understood. The third charged lepton, discovered at SLAC in 1975, and the fifth, or bottom quark, emergent from the Fermilab discovery of the upsilon resonance, were totally unexpected particles theoretically. Thus there is no shortage of profound theoretical problems, even within the standard picture.

Physics in the 1980's According to the Standard Picture

The facilities proposed for the 1980's, both in the U.S. and in Europe, should provide much better and more decisive tests of the standard picture.

The number of crucial tests of QCD is small. The higher energy e^+e^- colliders are especially useful in providing such crucial tests. Fixed target lepton-nucleon scattering and, if possible, ep colliding beams are another class of experiments which provide especially clean tests. The higher energies available in the 1980's at the Tevatron and elsewhere will be especially useful.

The many successful tests of electroweak theory have all been at energies small compared to its natural energy scale, given by the masses of the $W^\pm$ and Z^o. The clear central goal is the direct observation of these particles. The large value of the predicted masses make colliding beam facilities a necessity. ISABELLE and the $p\bar{p}$ Tevatron collider are well suited to this task. Electron-positron collisions would be a rich, especially clean source of Z^o's, allowing the accurate determination of Z^o properties. This is a prime motivation for e^+e^- colliders.

Another goal of electroweak theory will be to measure the single parameter of the theory (the weak mixing angle θ_W) more accurately at low energies as well as at high. This can be done in a variety of ways at all the fixed target machines.

In addition to describing the force laws, the standard picture anticipates the existence of a sixth, top quark, as yet undiscovered. If the mass is less than 100 GeV, it should be found at LEP or some other e^+e^- collider. For masses larger than that, it appears feasible to observe this quark in pp or $p\bar{p}$ colliders such as ISABELLE or Tevatron.

In addition, a third neutrino associated with the tau lepton is anticipated. The Tevatron fixed target program provides about the only opportunity for producing and studying this particle. Such experiments appear difficult, but attempts are already being made and will certainly continue to be pursued.

There is the chance that the single Higgs boson of the standard picture will be discovered if its mass is less than about 100 GeV. This should be possible with e^+e^- colliding beam machines such as LEP. If the mass is considerably larger, the task may have to be left to later generations of accelerators.

Finally, in the standard picture the puzzling phenomena of CP violation is correlated with the equally puzzling issue of the origin of quark masses and mixings. It is necessary to find additional observable effects which test this general idea. Precision measurements of K decay, now underway at Fermilab and BNL, and intense measurement of bottom quark properties at CESR or PEP may provide new information of vital importance.

The Program of the 1980's Without the Standard Picture

It may be fair to say that while the 1970 physicist was too pessimistic about the progress which would be made in the 1970's, the 1980 physicist may be too optimistic. Particle physics has, without fail, encountered major surprises in every new energy regime it has entered. Even within the basic framework of QCD and electroweak theories, unanticipated phenomena may be found. There may be no top quark. There may be rare decay modes of familiar particles into unexpected final states. There may exist new kinds of particles which we cannot even imagine. All machines -- high energy and low; pp, $\bar{p}p$, e^+e^- and ep -- have the potential for providing future surprises. However, very heavy particles may be produced only with high energy colliders. Any new particles with electroweak interactions will be produced at an e^+e^- collider. Any new particles with strong interactions will be produced at a pp or $\bar{p}p$ collider. Who knows what new particles will be produced in an ep collider, for no such machine has ever been built. Each kind of machine complements every other.

Even within the framework of QCD and electroweak theory, it may happen that quarks and/or leptons are themselves composites of simpler constituents -- a possibility which is suggested to some by the proliferation of families of quarks and leptons. Experiments on all three basic kinds of colliders, e^+e^-, ep and pp (or $\bar{p}p$) should be sensitive to compositeness on a very short distance scale.

Finally, we still do not know whether QCD and electroweak theories are really correct or, if correct, only have a restricted range of validity. Remarkable but controversial cosmic ray experiments have hinted at the existence of strong-interaction phenomena at collider energies, phenomena which could signal a radical departure from the picture at present energies. The existence of isolated, fractionally-charged quarks would likewise require, at the least, a major revision of QCD as it now stands.

The weak interaction likewise may not be described within the general gauge-theory framework which is now very widely accepted. Even in the unlikely possibility that the weak bosons do not exist, there is good reason to believe that the natural energy scale of the weak force must lie below 1 TeV, an energy scale accessible to the Fermilab $\bar{p}p$ collider. There are strong reasons to believe this scale in fact to be less than 200 GeV, a regime which is accessible to ISABELLE and especially to ep colliders.

Balance and Diversity of the Program

It should be apparent from the above discussion that diversity of approach is an invaluable asset. Many phenomena are best seen with probes involving electrons, both e^+e^- and ep. Others benefit from the extra center-of-mass energy available in proton-proton or proton-antiproton colliders. Still others require the high intensity and variety of beams in fixed-target accelerator facilities, high energy and low; while yet others, such as the proton decay experiment, do not need an accelerator at all.

Therefore, whether or not the present optimism is vindicated, a balanced and diversified High Energy Physics program for the 1980's will be a rich one and will have a basic impact on our view of the subnuclear world.

Intellectual and Technological Applications to Other Fields

Although the primary justification of High Energy Physics is the drive for a deeper understanding of the fundamental nature of matter there are number of direct and indirect by-products of the field.

The enormous development of the theory of quantum fields has had major applications to solid state theory and to condensed matter research. The progress of elementary particle physics has provided tools for astrophysics and cosmology, for example in the study of neutron stars and in the extrapolation back to the early stages of the big bang universe. The recent development of a precise theoretical structure has reawakened the interest of many pure mathematicians in physics and led to fruitful exchange and mutual stimulation. The development of a quantitative theory of the strong interactions provides a foundation for nuclear theory. Indeed an increasing fraction of the present program in nuclear physics deals with QCD and electroweak phenomena. This might eventually lead to the prediction of novel nuclear phenomena or new applications.

The experimental accomplishments of the last decade were made possible by a remarkable series of technological developments. Some of these are listed below:

(1) The science and technology of producing, focusing, transporting, accelerating, and storing high energy particle beams has made great progress in the last decade. It is interesting to note that it was the high level of this technology which has made the heavy

ion approach to inertial fusion an exciting and hopeful approach to fusion energy. Indeed, the heavy ion ignited inertial fusion idea was initiated and developed by high energy accelerator physicists. The scanning electron microscope was inspired by methods of high energy particle beam technology and indeed was developed by a high energy physicist. We also note that the physics of particle beams has been applied to research in cancer detection and treatment.

(2) The applications of the synchrotron radiation emitted by the circulating beam in an electron storage ring were developed to a high level because of the importance of this device in high energy physics research. This has led to the controlled use of synchrotron light for a very large and important class of investigations in several fields, solid state physics, surface physics, biology, and micro-circuit fabrication.

(3) The development of particle detectors has continued at a remarkable pace. The resolution and data handling capability of high energy physics research instruments have increased enormously in the last decade. Multiwire proportional and drift chambers were developed and widely used. One important application of these instrumentation techniques is in medical tomography. In addition to the direct application of the detectors themselves, the ideas and system concepts in High Energy Physics are of value for applications. We note that the basic principle of the CAT scanner was developed by a physicist who was a High Energy Physics principal investigator at the time.

(4) The ongoing development of large-scale superconducting magnet projects has been, and continues to be, pioneered by high energy physicists. This technology has led to successful application in the construction of large magnets for the generation of electricity through magnetohydrodynamics (MHD) as well as magnets for fusion energy, both for confinement and for ohmic heating. The development of large-scale superconducting systems may also have important applications to the efficient transmission of electrical energy.

(5) The Winston cone, originally developed for a Cerenkov counter at ANL, has found application in the field of solar energy as a moderately concentrating, nontracking collector. This collector design has potential for good performance at low cost and is currently under industrial development.

High Energy Physics, both experimental and theoretical, may be expected to continue to generate new developments of importance on a wider scale such as those indicated above.

SECTION III

THE CURRENT EXPERIMENTAL HIGH ENERGY PHYSICS PROGRAM

The U.S. High Energy Physics program depends on particle accelerators and storage ring devices located at three national Laboratories supported by the U.S. Department of Energy (Brookhaven National Laboratory, Fermi National Accelerator Laboratory, and Stanford Linear Accelerator Center) and the Cornell University Newman Laboratory of Nuclear Studies supported by the National Science Foundation. Because the various facilities involve different kinds of particles, particle energies and intensities, and targets, the experiments performed at these facilities address many different aspects of High Energy Physics. This section outlines some of the physics activities and opportunities associated with the accelerators operating and under construction in the U.S. High Energy Physics program. Important experimental research supported by the U.S. program, but not associated with high energy accelerators, is also identified.

Fixed Target Program at BNL

The fixed target experimental program at Brookhaven National Laboratory is based on the 30 GeV proton synchrotron, the AGS, which provides not only intense external beams of high energy protons but also secondary separated beams of pions and kaons and very good neutrino beams. Many major discoveries have been made with the AGS throughout its distinguished history. Some of the highlights are: two neutrinos; the "Omega Minus" hyperon; CP violation; the J particle; and the charmed baryon. The physics program now focuses on: studies of the weak interaction via neutrino interactions and K meson decays; the search for neutrino oscillations; hadronic interactions via spectroscopy, dynamics and polarization studies; QED via measurements of muonic atoms with helium nuclei; and a strong program of intermediate energy nuclear physics studying kaon-hypernuclei and K^- - and hyperon-induced atomic X-ray emission.

The AGS has the highest average proton intensity available. It provides up to 10^{13} protons per pulse every 1.2 seconds for neutrino physics and, with a one second flattop, every 2.5 seconds for electronic counter experiments.

Fixed Target Program at SLAC

The present SLAC linac remains the highest energy and highest intensity electron accelerator in the world. The SLAC accelerator also provides positrons and longitudinally polarized photon beams. In normal operation the SLAC electron beam has an energy of 23 GeV, while in the SLED I mode it has achieved energies up to 33 GeV.

The fixed target program is now centered on three facilities -- the electron spectrometers, the multiparticle spectrometer (LASS), and the hybrid bubble chamber facility.

The electron spectrometers have been used in a long and productive program that has led to some of the important discoveries of the past 10 years (viz. the deep inelastic electron scattering and the parity violation measurements). The bubble chamber has had a very productive program of photo-production and hadronic interaction studies and is now being used to study charmed baryon production by monochromatic photons, and also to measure their lifetimes. The LASS spectrometer is being used for a programmatic study of the systematics of strange quark spectroscopy.

Future experimental topics involve more polarized electron scattering experiments to measure parity violation at large q^2, and Møller scattering and very high sensitivity polarized photoproduction experiments.

400 GeV Fixed Target Program at Fermilab

The physics program at Fermilab uses a 400 GeV proton synchrotron. Proton beams of $> 2 \times 10^{13}$ particles per pulse are produced with a repetition rate of one burst each 10 seconds. Proton beams can be directed simultaneously onto several targets, producing secondary beams of hadrons, muons, photons, and neutrinos. As many as 15 experiments can be run simultaneously, resulting in a program of great diversity.

In its early days (1972-74) the program was dominated by the exploration of a new energy regime. Experiments ranged from extensions of hadronic physics at lower energies to searches for quarks and monopoles. As the machine matured, the main thrust of the experimental program focused on the study of hadronic substructure, the interaction of subnuclear constituents with one another, and the investigation of new forms of matter such as charm.

A major discovery of the Fermilab physics program was that of the upsilon resonance, leading to its interpretation in terms of a fifth quark, the b-quark. The substructure of nucleons has been studied by means of muon and neutrino scattering. The structure of pions has been studied by means of dimuon production in pion-nucleon collisions. Static properties of hadrons such as hyperon magnetic moments have been measured. This program has also revealed unexpected dynamical effects such as the polarization of hyperons produced at large transverse momentum. Numerous measurements on the production and properties of charmed particles have been carried out. Notable are direct measurements of the lifetime of charmed particles.

SPEAR

The SPEAR electron positron storage ring at SLAC operates with a center-of-mass energy region between 2.5 and 8 GeV. The research program at SPEAR has produced a large number of spectacular results including the discoveries of members of the psi family, the charmed particles, the tau lepton, and the production of two-jet hadronic structures. With the commissioning of the higher energy storage ring PEP, the SPEAR facility is now operating 50 percent of the time for particle physics, the other 50 percent going into research with synchrotron light. There remains a rich area of work in the SPEAR energy range that focuses on the systematic study of states involving the c-quark. The recent discovery of the "charmed eta" proves the continuing vitality of the SPEAR program.

CESR

The Cornell electron-positron storage ring CESR, designed to operate at center-of-mass energies between 8 and 16 GeV, obtained its first luminosity in September 1979. During the subsequent 6 months, experimenters at both CESR interaction regions have obtained important results, including confirmation of the three narrow upsilon states and the discovery of what may be a broader upsilon resonance, probably above threshold for the pair production of mesons with b-quarks. The detailed study of states involving b-quarks, both mesons and baryons, including spectroscopy and decay properties, represents a rich and unexplored area to which the CESR facility has an ideally matched energy domain.

PEP

The PEP e^+e^- storage ring at SLAC, which covers the range from about 8 to 36 GeV in center-of-mass energy, is just in the process of commissioning. Although the same energy region has already been given an exploratory study at the German storage ring PETRA, one may expect that increased luminosity and a wide variety of detectors will allow the PEP program to make substantial advances in such areas as tests of QCD, studies of jet structure and fragmentation, measurements of interference between weak and electromagnetic currents, and searches for new particles.

Fermilab 1000 GeV Superconducting Accelerator and Storage Ring

A major new accelerator storage ring complex is being developed at Fermilab by means of adding a string of approximately 1000 superconducting magnets placed directly beneath the present 400 GeV accelerator ring. This allows for a variety of thrusts: large power cost savings can be realized by running the present accelerator up to 150 GeV and then transferring the beams into the superconducting ring for attaining 400-500 GeV (Energy Saver mode); the superconducting ring will also provide the opportunity to reach very high energies by colliding beams head on. This will involve producing and accumulating antiprotons and then simultaneously accelerating these antiprotons and a counter-rotating beam of protons in the superconducting ring to collide with each other at a center-of-mass energy of 2000 GeV (Tevatron I); the superconducting ring also allows for accelerating the primary protons up to 1000 GeV and then extracting them, thereby doubling the energy of the fixed target program involving neutrino, muon, and hadron collisions with high intensities and energies (Tevatron II).

The physics opportunities provided by proton-antiproton collisions in the superconducting ring are unique; nowhere else in the world will collisions at such high energies be available for experimental study. These collisions are expected to provide much new information on constituents of hadrons (quarks), the heavy carriers of weak and electromagnetic forces ($W^{\pm}$ and Z^{0} particles), and the interactions among these particles. Furthermore, this project represents a major thrust into the unknown with good prospects for major discoveries. The higher energies made available by the superconducting ring to the fixed target program make it possible to produce and study particles in new ways not possible with the 400 GeV program and to extend to higher energies many of the studies now carried out at 400 GeV.

ISABELLE

The major authorized project for the future is the 400 GeV on 400 GeV colliding proton machine ISABELLE at BNL. ISABELLE will provide an order-of-magnitude increase in energy available for new-particle production by proton-proton collisions. It represents a major expansion of the frontier of High Energy Physics. An extremely attractive attribute of ISABELLE is its very high intensity which will allow rapid data accumulation as well as exploration of rare processes. ISABELLE will therefore allow searches for the $W^{\pm}$ and Z^{0} particles, possible new members of the quark family, and most important, for the completely unexpected, which historically has accompanied the advent of nearly all new machines.

Non-Accelerator Projects

High Energy Physics never did and never will consist solely of experiments done at conventional accelerators. Much of importance can only be discovered in other ways. Current theories predict that protons live approximately 10^{31} years. Several U.S. experiments will test this prediction. These novel experiments involve very massive underground detectors. They may find proton decay, or reveal surprises such as new cosmic ray phenomena, unstable superheavy particles, or neutrino oscillations. Neutrinos may have mass, and may oscillate, one kind into another. This can be studied in many ways: accelerator neutrinos, intense radioactive sources, beam dumps, the electron spectrum of tritium beta-decay. There already exist hints of effects, and better experiments must be done. Reactors can be used to measure the neutron electric dipole moment and to look for nucleon-antinucleon mixing. Theory suggests the possible existence of unobserved forms of stable matter. Very heavy monopoles may be incident as cosmic rays; very heavy isotopes may be present on Earth. A variety of experiments is called for. Particle physics and astronomy have become closer than ever. Solar neutrinos and neutrino astronomy can tell us as much about neutrinos as about stars.

SECTION IV

ACCELERATOR FACILITIES

This section briefly describes the recently completed accelerator facilities, projects now underway, and some ideas for the future which have been discussed.

RECENTLY COMPLETED FACILITIES

CESR

In the fall of 1977, the National Science Foundation approved an e^+e^- colliding beam facility, CESR, at Cornell University. The design energy was 8 GeV per beam, expandable to 10 GeV. This machine was completed 1 year ahead of the contract period and first luminosity from colliding beams was obtained in September 1979, 2 years after approval of the project. Costs for the storage ring, a large magnetic detector, and computing facility were $400K less than the total budget of $20.7M.

The energy region for which this machine was designed has turned out to be a very interesting one because of the upsilon and related particles, and a productive experimental program is now underway, with the first interesting results already published.

PEP

A major e^+e^- colliding beam facility, PEP, was completed at the Stanford Linear Accelerator Center in April 1980 on time and at cost. This machine was designed for maximum luminosity at 15 GeV per beam and maximum energy 18 GeV, expandable to 24 GeV if a substantial upgrade were to be carried out in the future. This machine was constructed as a joint SLAC/LBL project.

In view of the 2 years earlier operation of the similar machine, PETRA, at DESY, the luminosity of PEP will be of critical importance. It is too early to tell what this luminosity will be but there is expectation that it will exceed that achieved at PETRA.

The experimental program at PEP will begin soon; a number of detectors are installed and others will be completed during the coming year.

PROJECTS UNDERWAY

Fermilab Energy Saver/Tevatron

(1) General

Although the Fermilab Energy Saver-Tevatron I-Tevatron II projects have been defined as three separate construction projects they represent one integrated program to be accomplished in three phases: the Energy Saver and related 500 GeV programs, the 1 TeV fixed target program, and a $\bar{p}p$ collider program at 1 TeV on 1 TeV. This is clearly an ambitious program involving significant R&D, and it will require vigorous effort by the staff and users of Fermilab for effective achievement. It is one of the most exciting efforts currently underway in the U.S. High Energy Physics program. It is important that the authorization of the two Tevatron projects proceed in a timely fashion in order that the Fermilab management can implement an optimal program utilizing the Energy Saver and Tevatron facilities. It is recognized that surprises may be in store due to the lack of operating experience with major large superconducting magnet and cryogenic systems in accelerator applications. These new challenges will have to be met with vigor and additional R&D support may be required. Schedule risk remains high.

(2) The Fermilab Energy Saver

This project is underway at Fermilab and is scheduled for completion in 1982. After experiencing a number of difficulties in the R&D program for the development of production-quality magnets,the Fermilab personnel believe they will soon be ready to proceed with full production of the 800 cryostatically-encased superconducting dipoles. They have produced some 200 production-quality coil structures which they believe are satisfactory. Recently, a problem occurred with the assembly of these coil structures in the cryostats, which they now believe has been solved so that full-scale production on one shift per day can begin soon. In order to meet the schedule, two-shift production must begin in the fall of 1980.

The Subpanel feels that considerable progress has been made in overcoming the problems of constructing superconducting magnets, but it is concerned that more problems may be encountered which will require additional R&D efforts for their solution. Such problems could result in a delay in completion of the project.

Fermilab has developed a number of critical quality assurance and magnet measurement programs to ensure a satisfactory design. This has been a difficult pioneering effort in an area of critical importance in the development of future accelerators and in the development of other large superconducting magnet and cryogenic systems. Fermilab has also tested strings of magnets and cryostats in a variety of configurations to ensure that their design for magnet protection, controls, cryogenic and other systems will perform adequately. They are in the process of installing a string of accelerator-type superconducting magnets to bring the present 400 GeV proton beam into the Meson Laboratory. This system, operating continuously with a particle beam in an environment similar to an accelerator, will be another important systems test. Other elements of the Energy Saver, i.e., the refrigeration system, and the rf system, are under construction, and testing and commissioning of major subsystems are occurring.

Operation in the Energy Saver mode is scheduled to begin in 1982 with energies up to 500 GeV, resulting in significant savings in power compared to the present fixed target operation.

(3) 1 TeV Fixed Target Program

The Tevatron I-Tevatron II program will allow the superconducting accelerator to reach proton energies near 1000 GeV with adequate repetition rate and will provide extracted 1 TeV proton beams and experimental area facilities for a 1 TeV fixed target research program.

This facility will provide the highest energy secondary particle beams in the world for many years. The opportunities afforded for experimental investigation of important fundamental issues in the study of the basic constituents of matter are rich and unique.

The research and development work leading to this design has been extensive. A Summer Study was held in 1976, followed in the last year by workshops treating the individual experimental areas. There are many technical papers available on particular studies. At every stage of the work leading to this design report, experimenters and users from the entire national and international particle physics community have participated and contributed.

The 1 TeV fixed target program, Tevatron II, includes:

(a) The construction of a slow-extraction system for 1 TeV protons.

(b) Improvement of the external beams switchyard to 1 TeV and the provision for 1 TeV targeting in the present external experimental area.

(c) The construction of facilities for new secondary-beam enclosures and support facilities in each external experimental area designed to take full advantage of the fixed target physics opportunities of the Tevatron. The total number of secondary beams will not be increased because the new beams will replace existing ones, but the energies and intensities of the secondary beams will be significantly increased.

This project will build on the experience achieved with the Energy Saver program in accelerator, extraction, and proton beam targeting systems.

(4) $\bar{p}p$ Collider

Fermilab plans to use the 1 TeV superconducting ring for beam storage of counter-rotating antiproton ($\bar{p}$) and proton (p) beams. Colliding beam experiments can then be performed in a special straight section, where beams are focused to very small cross section to enhance the interaction rate (low beta section).

The maximum reaction energy of 2000 GeV between colliding particles and the expected high interaction rate expressed by the luminosity $L = 10^{30}\ cm^{-2}s^{-1}$ will make this $\bar{p}p$ collider a truly unique facility in the world for many years to come.

The most difficult part of the project is the collection of intense current of antiprotons in 12 bunches with 10^{10} antiprotons in each of them. Antiprotons are produced by bombarding a special target with 80 GeV protons. Because the production efficiency is very low (3×10^{-6}), antiproton accumulation is a slow (5 hours) process. Antiprotons created in the target with energies of about 4.5 GeV are injected into a special precooler ring, where particle momentum spread is reduced by so-called "stochastic cooling." This makes it possible to decelerate the antiprotons to an energy of 200 MeV and inject them into a second special ring (the "cooler ring") where they are accumulated and their oscillations and momentum are further reduced by interaction with an intense electron beam ("electron cooling"). When the required total number of antiprotons (12×10^{10}) has been accumulated, they are transferred to the main proton synchrotron, accelerated to 150 GeV, extracted from the main ring and injected, in the form of 12 bunches, into the superconducting Energy Saver ring. After an equal number of proton bunches (each with 10^{11} protons) has been injected in the opposite direction in the Energy Saver ring, protons and antiprotons are slowly accelerated to the maximum energy of 1000 GeV.

Because of the long accumulation time for antiprotons, the reliability of the accelerator system, especially that of the "cooler ring," is of major concern. There are also new techniques involved, which have been tested to some extent but which need further investigation. The most critical components of this colliding beam scheme are being studied extensively at present. The production target for the $\bar{p}$ has to withstand a beam burst of 1×10^{13} protons at 80 GeV with a burst length of 1.6 microseconds and a beam spot of approximately 0.4 mm diameter. A number of studies have been conducted to address this problem and an appropriate R&D program is planned. Alternatives such as sweeping the beam over the target are being considered.

A version of a 200 MeV electron cooling ring exists at Fermilab including an electron gun for the electron cooling. First electron cooling experiments will be done in the near future. In the same ring longitudinal and transverse cooling experiments with stochastic cooling have already been carried out successfully.

In the main ring, beam storage studies and rf manipulation experiments with the goal of bunching a large fraction of the beam at 80 GeV into 1/13 of the circumference are underway.

In 1979 a Conceptual Design Report of the $\bar{p}$-source was published and submitted to DOE for FY 1981 approval.

Currently Fermilab plans to build two experimental areas for $\bar{p}p$ collisions. Fermilab, together with ANL, LBL, the University of Wisconsin, and the Institute for Nuclear Physics at Novosibirsk, USSR, has formed a collaboration supporting the R&D and construction efforts for the $\bar{p}p$ project.

Such an advanced accelerator project needs commensurate R&D efforts. To date, Fermilab is heavily engaged in the Energy Saver R&D and construction. As a result, the $\bar{p}p$ Collider R&D has slipped. It is hoped that the Collider can soon receive the necessary attention.

In comparison to the CERN $\bar{p}p$ scheme, the Fermilab design has a number of advantages. Due to the larger momentum of the colliding beams (2 x 1000 GeV compared to 2 x 270 GeV) and the smaller emittances, the design luminosity approximately 10^{30} cm^{-2} sec^{-1} can be obtained with one-fifth of the antiproton current. Hence the accumulation time is expected to be 5 hours rather than 24 hours as required at CERN.

There are a number of potential improvements such as increased solid angle in the $\bar{p}$ acceptance, increased target luminosity, reuse of the antiprotons, etc, which could increase the number of $\bar{p}$ in the same collection time and hence increase the luminosity or reduce the collection time and ease operational problems.

The uniqueness of this project and its rich research potential, particularly the very high energy achievable, make it most desirable to proceed expeditiously.

BNL ISABELLE

ISABELLE is an intersecting ring accelerator and storage ring facility designed to collide opposing beams of protons at center-of-mass energies between 60 and 800 GeV. The design luminosity at the "standard" crossing points is 2×10^{32} cm^{-2} sec^{-1}. By provision of special focusing elements at the crossing points, the design indicates that a peak luminosity of 10^{33} cm^{-2} sec^{-1} might be achieved.

The accelerator itself consists of two independent rings of superconducting magnets of 50 kilogauss peak field, arranged roughly on a circle of 3834 meter circumference in such a way that the opposing beams can be brought into collision at 6 places around the ring.

The status of the project is as follows:

(1) Civil Construction

ISABELLE construction was authorized in FY 1978. A substantial portion of the conventional (civil) construction is now underway. One-third of the tunnel is almost complete. Work is underway on most of the rest of the tunnel.

Two of the interaction areas are now under construction. The civil construction is on schedule at cost. The tunnel is scheduled to be finished in mid-1981 with the rest of the civil construction to be completed in early 1983. Experience to date indicates that this schedule is realistic.

(2) Standard Accelerator Components

Design and model work is now underway on the accelerator components other than the magnet and refrigeration system, which are discussed below. Some of these items are injection and ejection equipment, rf stacking and accelerating systems, vacuum system, magnet power supplies, and instrumentation and control systems.

(3) Refrigeration System

Extensive work on the refrigeration system has been underway for some time. Heat loads for most major components have been measured (transfer lines excepted) and found to be within design specification. Considerable R&D on some of the critical components such as special heat exchangers, oil removal system, etc., has been carried out. The design for the main cold box for the system is underway. The system is designed to rely on turbine expanders. Small, gas-bearing expanders in test refrigerators have given considerable trouble but, from other experience at BNL, the ISABELLE group feels that the larger turbines for the main refrigerator, which use oil bearings, will be more reliable. This has yet to be demonstrated. No large-scale tests of the cryogenic system have been carried out under realistic accelerator operating conditions.

(4) Superconducting Magnets

The current ISABELLE design calls for 732 dipoles of 5 Tesla (T) field strength and 348 quadrupoles of $60Tm^{-1}$ gradient. The manufacture of the dipole magnets has encountered unexpected difficulty. The magnets produced so far do not reach the design maximum fields or field quality. In response, BNL has instituted an R&D program to analyze the magnet design with the aim of correcting design deficiencies. A major element in this program is the set-up of manufacturing facilities at BNL for the fabrication of full-scale magnets. By experimental study of magnets built with tightly controlled design variations, supplemented by theoretical analysis, the magnet development group hopes to uncover those features of the current design which result in substandard performance. The tooling available could be capable of manufacturing four sets of dipole coils per month. The plan is to produce at least two sets per month at BNL and ultimately to have most of the magnets produced by industry. This will require careful quality control on the production magnets. In addition to the field strength and training problems, the Laboratory recognizes that the problems of magnetic field quality and quench propagation must also be resolved.

The quadrupole magnets built so far at BNL are reported to be successful in terms of overall performance. Pilot production of quadrupoles at the BNL site is to begin shortly. Fifteen quadrupoles will be manufactured and tested thoroughly before proceeding further with production.

In an attempt to bring more experimental and analytical effort to bear on the magnet problems the organizational structure of the ISABELLE construction staff has been changed and the number of personnel in the magnet program has been increased during the past 2 years almost fourfold, up to 130. Outside help has been sought and some outside experimental or analytical work is being done at LBL and the MIT Magnet Lab. Additionally, the BNL management has set up an internal mechanism for exploring alternative magnet designs, should the problems with the current design prove intractable.

At this time, the BNL management hopes that pilot production of dipoles can begin in mid-1981. Current information from the R&D program is insufficient to assess the likelihood that this goal can be met. If it is not, the ISABELLE project will encounter significant delays.

For achievement of overall project goals, the BNL management may need to enhance technical leadership in a number of areas. In addition, a broader participation in ISABELLE by other BNL staff, particularly from the Physics Department and the AGS, and, perhaps, broader use of personnel from other Laboratories and industry may be necessary. BNL should continue to strengthen the practice of periodic in-depth technical review, involving outside expertise.

Although significant steps have been taken and are being taken by BNL management to solve ISABELLE problems, the very nature of the hardware involved and the lack of experience anywhere with systems of similar technological complexity increase the likelihood of further schedule slippage and the need for additional R&D. In meeting this very difficult challenge, technical leadership will be crucial.

PROPOSALS AND IDEAS FOR NEW FACILITIES

SLAC Linear Collider

(1) Description of the Project

SLAC has proposed to build a novel linear collider device (the Stanford Linear Collider or "SLC"), which would provide electron-positron collisions with an interaction energy (center-of-mass) of 100 GeV and a luminosity of 10^{30} cm^{-2}sec^{-1}. Based in large part on the SLAC linac, the SLC would provide colliding 50 GeV beams rapidly and at moderate cost.

The proponents point out that the SLC could be the first of a new class of electron-positron colliding beam facilities. It now seems relatively certain that circular colliding beam storage rings for electrons would not be feasible at center of mass energies above 300 GeV or so. On the other hand, there are no known fundamental technical limits to the energy which might be reached with linear collider devices until one reaches center-of-mass energies much beyond 600 GeV. The costs of a linear collider will be less than those of a circular machine at some beam energy in the few hundred GeV region. Nevertheless, the costs and power consumption of a linear machine are also likely to be very high. One of the goals of the SLC project is to demonstrate the feasibility of the linear collider concept, and to begin exploration of ways to reduce the expected costs and power.

In the operation of SLC, two microscopic, high-intensity particle bunches -- one of electrons and one of positrons -- are accelerated to an energy of 50 GeV in the Stanford two-mile accelerator after which the two bunches are separated, and transported in separate, reversed-double-bends (like "question marks") so as to be brought into a head-on collision. At the collision point -- where the high energy interactions are studied -- the bunches are compressed by focusing magnets to diameters of a few micrometers in order to achieve the desired interaction rate. Bunch pairs arrive at the collision point at the basic pulse rate of the linear accelerator (180 per second), and after the collision the electrons and positrons are disposed of in a dump.

The SLC requires major modifications and additions to the existing SLAC facilities.

(a) The linear accelerator must be modified to obtain higher electric fields, and thus to reach a peak electron energy of 50 GeV (to be compared with the present value of 33 GeV).

(b) Special sources of both electrons and positrons must be developed and built to provide the two microscopic beam bunches. Each bunch must have a duration of a few picoseconds and a current of a few kiloamperes, providing 5 x 10^{10} particles (to be compared with the current maximum performance of about 2 x 10^{9} electrons in a single bunch).

(c) The instrumentation and control system for the steering of the beam along its axis, and for the focusing of the particles along the full length of the accelerator must be upgraded in a major way to permit acceleration of the two particle bunches down the length of the accelerator without an unacceptable increase in their lateral sizes.

(d) The reverse-bend magnetic channels (which bring the bunches into collision) and the tunnels to house them must be designed and constructed.

(e) An experimental hall and experimental physics apparatus must be provided at the intersection point.

Stanford has proposed to build the SLC in a period of about 2-1/2 years beginning in October 1981 at a cost of $63M (FY 1980).

(2) Appraisal of the Project

The Stanford Linear Collider is a highly imaginative and innovative project which will involve a large number of diverse and novel accelerator developments -- from the realization of the very high-intensity sources of electrons and positrons to the achievement of the microscopic beam sizes at the interaction point. The Subpanel did not find any fundamental difficulties with the concept and proposed design during the short time the design report has been available. It believes that many of the required technical developments can be made in a straightforward manner. Some developments will require a high level of intellectual and technical effort to reach the required performance.

One major concern would be the capacity to accelerate a high intensity bunch along the full length of the accelerator with no significant increase in the effective dimensions of the bunch. Another concern is that relatively minor technical imperfections in each of the several components of the SLC would so reduce the overall efficiency in the acceleration and transport of the electrons and positrons that the SLC would fail to reach the design luminosity -- which some feel is perhaps already marginal. On the other hand, the SLC proponents have pointed out several areas in the system in which their evaluations are conservative and where additional factors of two or three in luminosity may be achieved after further study and development.

In summary, the Subpanel views the SLC as a fascinating and challenging new departure in accelerator technology which might in the near term provide, at relatively low cost, electron-positron collisions at an energy of 100 GeV, and which might well demonstrate an avenue which could possibly reach energies of the order of 1 TeV in the distant future.

The Subpanel has found no basic technical flaws in the SLC proposal and is impressed by the large amount of new knowledge about significant extensions of current techniques that should and would be gained if the project were to proceed. The Subpanel sees important opportunities for making advances in many aspects of accelerator science, and the intriguing chance of exploring methods which might lead the way to the study of electron-positron collision at very high energies.

The Subpanel believes that it is premature to attempt a detailed comparison of the SLC project with the colliding beam storage ring being contemplated by Cornell.

Cornell e+e- Storage Ring

Cornell has produced a preliminary design of an electron-positron colliding beam storage ring which would provide a maximum energy of 50 GeV per beam and a luminosity of 3×10^{31} cm^{-2} sec^{-1} in each of the 4 interaction regions. The project would achieve significant economies of power and size by the use of superconducting rf cavities and would extend the U.S. energy range of electron storage rings significantly beyond the PEP energy (18 GeV per beam), and into the region where the Z^o particle should be produced and where a very exciting field of physics will no doubt be found.

In a high energy storage ring, synchrotron radiation causes energy loss of the circulating electrons and necessitates continuous beam acceleration with powerful radiofrequency systems. Power consumption of these systems becomes one of the dominating aspects of these machines. The optimization of construction and operating costs depends critically on the magnitude of the required radiofrequency power. Superconducting rf cavities offer very large savings in operating costs and allow the optimized storage ring to be built with substantially smaller radius, thereby roughly halving also the capital cost. The Cornell Newman Laboratory of Nuclear Studies is leading the world in the development of superconducting cavities for circular accelerators. The maximum accelerating field of 3 MV/m for the proposed collider is substantially below the maximum field of 4.5 MV/m reached already in their test cavities. The proposed R&D effort in the next 2 years should allow the envisaged accelerating cavities to be tested in the CESR storage ring. Questions of parasitic mode damping, synchrotron radiation effects on the cavities and the economy of a large technical system could then be answered with confidence and might then lead to a formal proposal. This approach seems to be prudent and sensible. Since the proposed storage ring, with the exception of the radiofrequency system, is standard and well understood, such a machine -- after the R&D effort -- could be built with little technical risk. It would be unique in the U.S. and should -- because of its smaller radius -- produce larger luminosity than the proposed 86 GeV x 86 GeV storage ring at CERN in the region below 50 GeV x 50 GeV. The development of large superconducting accelerating systems would have a great impact on all new circular electron and even proton accelerators and may lead to large power savings, and it could have other applications.

ep Colliders

Interest in electron-proton colliding beams in North America has been expressed by a Columbia University group and independently by a group composed of several Canadian institutions. Both groups have considered a 10 GeV electron storage ring tangent to the Fermilab Tevatron ring, providing center-of-mass energies up to 200 GeV.

Since no detailed design of this facility has been presented, only a few general remarks are included here. The combination of high luminosity ($>10^{31}$ cm^{-2}s^{-1}) and polarized electron beams required by ep physics introduces severe problems of beam dynamics that should be carefully studied. Other problems requiring investigation are the beam-beam interaction, the stability of high current bunched beams, the depolarizing effects, and the comparison of various possible spin rotation schemes. Experimental information on some of these problems will become available during the next 2 years both in Europe and the U.S.

The possibility of using ISABELLE instead of the Tevatron should be seriously considered. There are potential advantages which compensate for the lower energy of ISABELLE, such as the longer ISABELLE straight section, the exclusive use of one interaction area, and the fact that ISABELLE does not have to supply beams for a fixed target program. All these facts and the overall impact of the ep program on the host laboratory should be evaluated in any future ep proposal.

Higher Energy Proton Accelerators and $\bar{p}p$ Colliders

Studies of multi-TeV proton accelerator/colliders are being pursued at Fermilab. Specifically, the studies are focused on a 5 TeV proton synchrotron/collider, 5 km in diameter and using 8 to 10 T superconducting magnets. A considerable amount of R&D work will be required before such high field magnets suitable for accelerators can become a reality.

SECTION V

UNIVERSITY PROGRAMS AND FACILITY UTILIZATION

Roughly 1,100 experimentalists and approximately one-half that many theoreticians are currently active in U.S. High Energy Physics. The vitality of this community depends primarily on the existence of accelerator facilities which allow one to probe interesting new phenomena, and on effective utilization of those facilities. We discuss here the efficiency of the present program, in terms of both accelerator utilization and manpower.

Universities

The High Energy Physics program depends upon the involvement of physicists at the major educational institutions of the United States, and benefits from the efficiency of experimentation which is possible at large central Laboratories. The diversity of initiatives, competition among ideas, and high quality of talent that results from the participation of University groups lends breadth and originality to the program and is fundamental to it. Most important, the participation of students and University scientists in High Energy Physics is essential to the future of the field, and contributes to the vitality of the University community and the education received by students of all the physical sciences.

There are, however, increasing problems which confront the University groups. It is essential that the University researchers be able to spend a significant amount of time at their home institutions so that they may interact effectively with students, both graduate and undergraduate, and with other faculty. Hence, while the running of an experiment and data-taking must occur at the Laboratories, the design, building, and testing of equipment, and the extensive data analysis required to yield physics results is often most profitably performed at the Universities. This approach has become increasingly difficult, however, for several reasons. First, the computational facilities of the Universities have not kept pace with evolving needs, forcing many groups to reside at the Laboratories long after data-taking is complete as they perform the analysis at Laboratory facilities. Several groups have managed much of their analysis at home institutions very efficiently, using small, but powerful DOE- or NSF-supported computers. The policy of distributed computing, where it is efficient and at a level that is in balance with the major facilities necessary at the Laboratories, should be encouraged. Secondly, the technical complexities of each experiment have grown to such an extent that the size of a typical team of physicists from a University and the time required for the pursuit of an experimental program have steadily grown. Roughly one-third of University research support is used for faculty salary supplements and another one-third for non-faculty personnel, principally post-doctoral fellows. The remaining support available for technical development has dwindled as indirect costs charged by the Universities and the costs of travel have soared.

One recent encouraging development occurred in the handling of PEP equipment funds. A significant fraction of the monies, after being allocated to each approved experiment, were channeled directly to the University groups; this enabled them to participate fully in the construction of the detectors.

While at the present time the bulk of support for High Energy Physics comes from DOE, it should be pointed out that the NSF role in supporting the University groups is vital. In a field such as High Energy Physics, where diversity of ideas and approaches is crucial, diversity and flexibility of sources of support are essential. Central participation by the NSF in the support of University-based high energy science provides this diversity and flexibility. Through its responsibility for the health of basic science in the U.S. and its special mission to nurture scientific education, the NSF is particularly well-suited to play an important role in particle physics at the Universities. At a time of high scientific opportunity in this most basic of sciences, increased NSF support for University-based High Energy Physics is urgent and will pay high dividends.

Fermilab

The 400 GeV fixed target program at Fermilab, with its wide variety of secondary beams, provides opportunity for many different types of experiments. In addition to the 106 experimental physicists employed by the Laboratory, approximately 750 physicists from 135 other institutions perform experiments there. While the physics program is still at the forefront of High Energy Physics research, it has now reached a stage where many of the experiments require a systematic approach utilizing elaborate detectors and extended periods of running time. The execution of experiments has become increasingly difficult, however, due to a shortage of operating funds. The shortage is aggravated by the large R&D expenditures required for development of the Energy Saver. The number of weeks of beam operation per year has decreased steadily from approximately 40 during 1973-1977 to 26 in 1980. In addition, a shortage of detector electronics, rigging, engineering and technical assistance to experimenters, and manpower to install apparatus, has increased the time required to assemble and test experiments. Other cutbacks have also been necessary: the Meson Laboratory, which includes more than 30 percent of Fermilab's facilities, was shut down for about one year, and the 15-foot bubble chamber, a unique facility in this country, has repeatedly been shut down to save money and manpower. The net result is that the number of experiments simultaneously using the beams has decreased from 12-13 during 1976-1978 to 5-9 during 1979-1980. Numerous experiments have been delayed one-to-two years due to lack of beam or adequate support. This is frustrating for senior physicists and is very demoralizing for graduate students and research associates. Further frustration is produced when experiments, first initiated in this country, are outclassed by similar experiments in Europe which proceed more rapidly with superior instrumentation. Fermilab continues to produce excellent physics in the face of these difficulties, but the delays and frustration may severely compromise the attractiveness of the field to young physicists.

SLAC

The community of experimenters at SLAC has grown rapidly in recent years following the approval of the first round of PEP experiments. From a total of approximately 120 active experimental physicists at the laboratory prior to 1977 (including approximately 60 from SLAC and Stanford University), the community in residence has grown to around 270, with 210 coming from outside University groups. The institutions involved number 53, and include groups from Canada, Great Britain, France, Japan, and other countries.

The overall utilization at the laboratory has shrunk steadily over the past 10 years. This is generally a reflection of the pressure on operating funds in the U.S. program and, in recent years, of the need for operations money to prepare for the PEP experimental program. The main brunt of the cutbacks has fallen on the linac physics program, which is now down to 20 percent effective utilization, although the e^+e^- colliding beam physics at SPEAR has also been substantially restricted.

The laboratory has responded in various ways to the reduced operating flexibility. The general maintenance of plant and level of services have been cut. Space is very limited. In addition, the laboratory has instituted programmed "pauses" in some areas and closed down other facilities entirely. The policy of the laboratory has been to approve only those experiments which can be supported well enough to proceed effectively. This has resulted in approval of fewer experiments (for both SLAC and University groups) but satisfactory support and running time for those approved.

BNL

The AGS at Brookhaven has been one of the most successful accelerators; after 20 years of operation there is still a community of 30 physicists within the Laboratory, and approximately 160 users from other institutions who perform experiments at this facility. Thirty-four experiments have received beam time since January 1978. An important feature of the program is that it is still possible to perform interesting experiments with relatively modest detectors and small groups of people. The utilization of the AGS is low with only 25 weeks of operation per year. The operations program has been hit hard recently by a 60 percent increase in power costs. Typically, five-to-six experiments and two test beams are in simultaneous operation; the test beams are in very great demand and typically service 25 groups per year. The availability of electronics, rigging, and other support is limited and often leads to delays in the execution of experiments. The program has been cut back by closing down the 7-foot bubble chamber, delaying the construction of two new facilities, and canceling the development of a rapid-cycling, high resolution bubble chamber. Any further cuts in the AGS operation or support of experiments will further strain the program.

Cornell

Supported currently by the NSF, Cornell has built and operated a series of accelerators over a period spanning more than 30 years. Most recently, the 12 GeV electron synchrotron was shut off for 2 years to permit the construction of CESR, an 8 GeV x 8 GeV colliding beam storage ring. CESR began operation for physics in the fall of 1979 and now serves approximately 100 users from 10 institutions. Services provided to users are minimal and appropriate utilization of this unique facility will be difficult with the present level of support.

Summary

In summary, the utilization of existing accelerators is significantly lower than the optimal level, and support of University groups is marginal. The High Energy Physics program continues to produce excellent physics in spite of these difficulties but would benefit greatly from increased support.

SECTION VI

LONG RANGE RESEARCH AND DEVELOPMENT

The Subpanel has given some consideration to issues in long range R&D both for detectors and for accelerators.

LONG RANGE DETECTOR R&D

Role of Detector R&D

The advances in accelerator facilities in the last decade have been accompanied by a remarkable set of advances in the art and technology of particle detection and measurement. These include the development and universal application of on-line computer control and data acquisition for High Energy Physics; the development of multiwire proportional and drift chambers; the development of calorimetric methods for particle energy measurement; and the development of a number of large, multicomponent, integrated detection systems.

Although the accomplishments of the field, to date, have provided high energy experimentalists with the tools to carry out significant research on the new accelerator facilities that will come into operation in the 1980's, it should be realized that there are research areas of great importance which cannot be addressed without further advances in detector technology. Furthermore, even for problems which can be attacked within the current state of the art, significant improvements in the acquisition rate and quality of experimental information could be achieved by improvements and innovations in detector technology. It is worth noting that these comments apply, albeit to a lesser degree, to research with our existing accelerators.

Opportunities for Detector R&D

At the risk of some oversimplification, the problems facing the detector development of the coming decade can be grouped into two general areas. First, it will be necessary to deal with very high rates of detection, both to handle real events and, perhaps as importantly, to be able to reject background events which may vastly outnumber some special events of interest. Secondly, it will be necessary to have the capability to simultaneously detect and record large numbers of particles produced in the same event.

The list below includes some areas of detector technology which are important candidates for further R&D. This list is meant to be suggestive and certainly is not complete. It is useful to consider the detector field in terms of two general categories -- detector components and detector systems.

(1) Detector Systems

(a) Design of integrated, multi-component, large solid angle detector systems is an important element in the research at colliding beam facilities, and has increasingly become an important aspect of fixed target research.

(b) Data Acquisition Technology - To utilize the high data rates available at fixed target accelerators and in some colliding beam facilities, a great deal of innovative work remains to be done including such matters as hierarchical trigger processors, on-line data reduction, detector system-trigger and computer system interaction, etc.

(c) Data Reduction Technology - This includes off-line computer software (and hardware -- special processors, etc.). This area includes the development of algorithms, system emulators, Monte Carlo methods, etc., required to process efficiently and economically the data produced by high energy experiments.

(2) Detector Components

(a) Particle Identification - The development of various methods of measuring the mass of individual particles via combination of momentum, velocity, and/or energy measurement is an important area. Cerenkov methods, ionization energy loss methods, and shower development methods are among the promising approaches which have been and will continue to be pursued.

(b) Particle Tracking - The need for accurate, rapid registration of charged particle trajectories has been present in High Energy Physics experimentation for many years. It continues to be one of the pacing elements of detector design. Multiwire chambers and drift chambers have developed to a high state in recent years but they will have to be pushed to their fundamental limits to utilize efficiently the new accelerators. Completely new techniques (to high energy physics) such as arrays of "micro" solid state detectors, perhaps produced on the same "chip" which contains their data acquisition electronics, offer significant improvements in accuracy and data rate but require much basic R&D.

(c) Calorimetric Energy Measurement - The calorimetric method (total energy loss measured via sampling) for particle energy measurement has become an important tool in high energy physics research. The method is still in a state of rapid evolution and should and will be the subject of continuing R&D.

(d) Ultra-High Resolution Detectors for Short Lifetime Measurements - The full study of the physics of the new (charm and beauty) heavy quark systems requires the development of new high resolution detectors capable of resolving complex production and decay events with a spatial resolution of a few microns, with electronic trigger and high data rate capability. Promising avenues have been started -- small rapid cycling bubble chambers, and high pressure streamer chambers -- but this field is just beginning.

Conclusion

Detector research is, to some degree, carried on by all high energy physics experimentalists. However, the long range, basic developments which are discussed here are carried out by a relatively small number of Laboratory and University groups. These efforts should be encouraged and supported by the Laboratory and agency administrators.

Long Range Accelerator R&D

The Subpanel has considered the report of the HEPAP Subpanel on Accelerator R&D and endorses their report enthusiastically. The Executive Summary of the report of that Subpanel, which gives its conclusions and recommendations follows.

> Accelerator Research and Development has always been an integral and important part of elementary particle physics research. The physics output has been paced, to a large extent, by the improvement in performance of our accelerators. In the past 50 years, equivalent beam energies available to experimenters have increased by a factor of ten million as a result of accelerator R&D efforts. Not only has performance increased exponentially but unit costs have also been reduced significantly. Even in times of decreased real spending power for the field, significant energy increases were achieved.
>
> Despite our remarkable achievements in unit cost reductions, these reductions have not kept pace with the energy increases achieved. This has resulted in a limitation in the number and diversity of high energy accelerator facilities available to workers in the field. Further decreases in diversity would be necessary to continue frontier research were the pace of cost reduction produced by new accelerator technologies not to increase. This would have a serious negative impact on the scientific productivity of U.S. elementary particle physics. The challenge is clear: we must redouble our efforts to reduce unit costs sharply while

maintaining performance improvements through full exploitation of the potential of our current accelerator technologies and through invention of entirely new technologies and methods.

Historically we have spent approximately 10 percent of our operating resources on accelerator R&D activities. This includes both R&D applied to projects (RDAP) and long range R&D (AARD) which is not associated with a specific project. Today this fraction for all accelerator R&D is 14 percent, reflecting our heavy commitment to superconducting magnet development. The great majority of this is RDAP, as it needs to be . The Subpanel has determined that between 1 percent and 1-1/2 percent of operating resources are now devoted to AARD. If we are to meet the challenge of the future, more of our resources need to be devoted to this activity. We believe that an appropriate level for long range R&D would be up to about 4 percent of high energy physics operating resources.

It is obvious that no level of expenditure for AARD can guarantee that the historical trend in unit cost reduction can continue. Nevertheless, the Subpanel is convinced that without increased effort on AARD significant unit cost reduction is most unlikely. Thus we conclude that increased effort on AARD is an essential investment for the community at this time.

Recommendations

1. The high energy physics community should significantly increase its suppport for AARD. The Subpanel estimates, from an examination of significant areas of this research, that the level for AARD in the high energy physics accelerator field should be increased from about 1 percent to 4 percent of our current operating resources. This increase should be achieved within the existing administrative framework.

2. In reviewing accelerator R&D, the Subpanel has identified certain specific technical areas that should be emphasized in AARD. This list is not presumed to be complete, exclusive, or to indicate relative priorities. Rather, it is intended to be an indication of the challenges of AARD and to

underscore the importance of AARD to the field of high energy physics. This list of specific topics is:

(a) Development of very high field accelerator magnets and the evaluation of the practical limits of this technology. In view of the large scale of this enterprise and its uniqueness, the Subpanel recommends that this AARD effort be carried out as a collaborative effort among the laboratories having capability in this area. The Subpanel feels that this development of high-field magnets should be focused toward a specified accelerator goal.

(b) Development of liquid helium refrigerator systems with goals of improving efficiency and reliability and providing operation at reduced temperatures.

(c) Theoretical and experimental exploration of the limits to microwave linac gradients and to the peak powers that can be delivered in the S- to X-band regions.

(d) Basic physics and device development in superconducting rf accelerators.

(e) Theoretical and experimental studies of basic accelerator phenomena, particularly the beam-beam interaction and other performance-limiting phenomena.

(f) Search for and preliminary development of new accelerator schemes with high performance potential such as laser accelerators or other devices using ultra-high peak power with or without collective effects.

(g) New techniques and devices for manipulating very high power and/or very high energy beams.

(h) The general problem of increasing the brightness of particle beams with emphasis on cooling high energy beams.

(i) Development of new beam diagnostic techniques and devices.

3. Laboratory and University managements, aided by the agencies, should take specific measures to make participation in accelerator R&D practical and more desirable for high energy physics theorists and experimentalists not now so engaged. This could include payments of salary for sabbatical leaves and leaves of absence; and on-the-job training through seminars, summer schools, and the use of specially assembled tutorial and review materials. Also included could be expansion of University-Laboratory collaborations.

4. Laboratory and University managements, aided by the agencies, should take specific measures to facilitate cross-fertilization from other fields such as Plasma Physics, Lasers and Materials Science, and other accelerator-related activities. This could include long-term visits by outstanding individuals in these fields for the purpose of working directly with Laboratory staffs, giving tutorial seminars or lectures series, or participating in extended topical workshops.

SECTION VII

INTERNATIONAL COOPERATION IN HIGH ENERGY PHYSICS

The fundamental importance of High Energy Physics research is exemplified by the fact that many countries have chosen to support major efforts in the field. The Subpanel comments briefly on some of the collaborative aspects between these efforts and the U.S. program.

These international collaborations have benefited all participants. Not only has the scientific research been enhanced, but the intellectual exchange between the U.S. high energy physicists and their counterparts in other countries has often been the leading edge of broader scientific and cultural exchange.

For many years there has been collaboration on an experiment-by-experiment basis at U.S. and European facilities. United States groups have, in collaboration with European groups, carried out experiments at CERN accelerators and, individually, and in collaboration with European groups have carried out experiments at DESY. A similar pattern exists for the use of U.S. facilities by European groups.

The U.S. and Japan have also had a long-standing program of collaboration along the lines discussed above. Recently, the U.S. and Japan have enlarged this program to include a substantial and more formal program of collaboration in High Energy Physics which includes both physics experimentation and collaborative efforts in accelerator research and development. This program has added support, as well as collaborators, and has significantly enhanced the joint projects which are underway.

The U.S. and the U.S.S.R have had a long-standing program of collaboration in the fundamental properties of matter. The major component of this effort has been the program in High Energy Physics. This program is continuing but, for the present, at a reduced level.

Finally, we note that in the past year a substantial collaboration has developed between the U.S. and the People's Republic of China (PRC). Under the terms of this collaboration, U.S. scientists are providing design and review assistance for a 50 GeV proton synchrotron to be constructed in the PRC. In carrying out this collaboration a number of scientists and engineers from the PRC are in residence at leading High Energy Physics institutions in the U.S. In addition to the scientists and engineers specifically concerned with the synchrotron, a number of other experimentalists and theorists have also come to the U.S. for extended periods of study and research. The Subpanel notes that the PRC included the development of High Energy Physics among its top national priorities for the coming decade.

SECTION VIII

THE EUROPEAN PROGRAM IN HIGH ENERGY PHYSICS

The European program in experimental High Energy Physics is centered at CERN and DESY. CERN has 3,600 staff members, a yearly budget of approximately 600 MSF, and roughly 1,800 high energy physicists are involved in the program. The corresponding numbers for DESY are a staff of 1,100 and a yearly budget of approximately 130 MDM. Although DESY is a national laboratory, it is exploited internationally with groups from 11 nations and a total of 400 physicists participating in the program.

At CERN the activities are centered around the fixed target operation of the SPS. The $p\bar{p}$ colliding beam option is scheduled to start operation in the second half of 1981. At present a strong and vigorous program is being carried out at the ISR. However, in order to free funds and personnel for the LEP project, ISR is likely to be phased out with 1983 as the last year of operation. CERN has also a strong program in medium energy physics fed by the PS. Furthermore, CERN has decided to construct a low energy $p\bar{p}$ facility LEAR based on technology developed for the $p\bar{p}$ project. For its future program, CERN, strongly supported by the European Committee for Future Accelerators (ECFA), has decided to construct a large electron-positron colliding ring LEP. The formal proposal to construct LEP will be submitted to the CERN Council in June 1980 with approval expected no later than June 1981. It is intended to start the experimental program during the fifth year of construction.

The program at DESY is at present based on the electron-positron colliding rings DORIS and PETRA with the synchrotron used as an injector and to provide test beams. A study of HERA, a large electron-proton colliding beam facility, with a maximum center-of-mass energy of 314 GeV has been submitted to the German Government. If the project is approved in 1981, the electron ring will be finished in late 1986 and the proton ring in late 1988. HERA will be open to all experimental groups both inside and outside of Europe.

The following discusses the facilities mentioned above in more detail.

(1) CERN

(a) Fixed target operation of the SPS - This machine will be the mainstay of the experimental program at CERN and is hence given high priority. The maximum energy is 400 GeV; however, it is likely that it will be upgraded to 450 GeV with a long spill and maybe to 470 GeV with a short spill by 1982. The intensity, with two batches injected from the PS is now about 1.5×10^{13} protons per pulse (duration approximately 10 s) or 2×10^{18} protons delivered on target in a 10-day period. It will be increased to 3×10^{13} protons per pulse once additional radiofrequency power is available in the SPS in 1981.

The SPS has a very strong experimental program with emphasis on the experiments in the neutrino and the muon beams in addition to the experiments producing dimuons and heavy flavored hadrons. A total of 35 experiments are currently active.

A shutdown, lasting about one year, to install the $p\bar{p}$ facility will start in June 1980. After start-up the available time will be shared between the fixed target and the $p\bar{p}$ running. It is expected that of the order of 25 percent of the available time will be used on the $p\bar{p}$ program.

(b) The $p\bar{p}$ colliding beam project - A considerable effort has gone into this project with the aim of producing $p\bar{p}$ collisions at 540 GeV in the center-of-mass with a luminosity of about 10^{30} cm^{-2} sec^{-1}. The facility will have two interaction regions and is scheduled to come into operation in late 1981. In this project 26 GeV/c protons from the PS are used to produce 3.5 GeV/c antiprotons which are stored and stochastically cooled in an accumulator ring. After accumulating a sufficient number of antiprotons (it takes about 12 hours to accumulate 3 x 10^{11} $\bar{p}$), the cooled antiprotons are reinjected into the PS, accelerated to 26 GeV (above the transition for the SPS) and transferred, presumably in three bunches, to the SPS. Work has progressed well and is now far along on several aspects of the project. The initial cooling experiment (ICE) was completed successfully in 1978 for stochastic cooling and in 1979 for electron cooling. The accumulator ring is under construction and will be ready for testing toward the end of the year. A series of machine tests in the SPS has succeeded in storing high intensity bunches for periods up to 12-18 hours. The emittance growth observed is consistent with that expected from multiple gas scattering. The civil engineering work on the two experimental areas is well underway. To revamp the machine for $p\bar{p}$ collisions, finish the civil engineering, and install the experiments is a major undertaking. To this end the SPS will be shut down for approximately one year starting June 16, 1980, with the fixed target operation scheduled to resume in June 1981. A total of five experiments have been accepted. Of these, two are large facility type detectors which will be ready to take data at turn-on.

(c) ISR - At present the ISR has a strong experimental program with nine active experiments. The machine performs well with peak luminosities of about 6 x 10^{31} cm^{-2} s^{-1} in the low-beta intersection region at 62 GeV in the center-of-mass. A simple transfer line has been constructed which makes it possible to operate the ISR for proton-antiproton collisions with luminosities up to 10^{30} cm^{-2} s^{-1} in intersection 8 (which will be equipped with superconducting quadrupoles to produce a low beta) and about 10^{29} cm^{-2} s^{-1} in the normal interaction regions. This will make it feasible to carry out a proton-antiproton program starting in 1981 using existing detectors.

It is intended to give high priority to the ISR and its program before the operation must be phased out in 1983 to free funds and manpower for the LEP construction.

(d) Low energy physics - The medium energy program is fed by the PS. The emphasis is on antiproton and kaon physics at very low energies. As a spinoff of the $p\bar{p}$ project, CERN will construct a low energy antiproton facility LEAR. Cooled antiprotons from the accumulating ring will be reinjected in batches into the PS, decelerated to 0.6 GeV/c and transferred to a small storage ring LEAR. On the average, $\geq 10^6$ $\bar{p}$ s^{-1} will be available. In LEAR the stored antiprotons can either be accelerated to 2 GeV/c or decelerated to 300 MeV/c. With some extra cooling and an ISR-type vacuum, this could be reduced to 100 MeV/c. LEAR has different modes of operation. It can be run as a stretcher ring with 1000 s spill time delivering an external beam of 10^6 $\bar{p}$ s^{-1}. It can also be run as a storage ring, either as a minicollider ($p\bar{p}$ at 4.4 GeV) or as a source of $\bar{n}$ via the charge exchange reaction $p\bar{p}$ $n\bar{n}$. Compared to present external beams LEAR represents a large step in beam purity, intensity, and duty cycle.

(e) LEP - The proposal to construct a large electron-positron colliding ring at CERN has been well received. The formal proposal will be submitted to the CERN Council in June 1980 and approval is expected no later than June 1981. Phase 1 of LEP is now defined as an e^+e^- machine with a circumference of 30.4 km capable of reaching at least 100 GeV in center-of-mass. It will initially have four interaction regions but the access tunnels to the remaining four are included in Phase-1.

The program calls for collisions in the fifth year of construction, i.e., 1986. This earlier date, compared with the previous schedule, has been made possible by making a lean design including only components needed to reach Phase-1. It has also been decided to use the SPS as an injector instead of building a 22 GeV electron synchrotron. This can be achieved by lengthening somewhat the SPS cycle without any other interference with the fixed target program. (The SPS as an injector is, of course, incompatible with the $p\bar{p}$ mode of operation.)

The energy of LEP can later be extended to 172 GeV with normal conducting cavities or 260 GeV with superconducting cavities. CERN, in collaboration with other institutions in Europe, has launched a strong effort to produce superconducting cavities at 500 Mhz, a frequency suitable for large electron storage rings. Initial results have been very encouraging. The next step will be to test these cavities in operating conditions at DORIS -- i.e., high power test, synchrotron radiation, higher order mode losses, and so on.

It should be noted that, in principle, the protons in the SPS can collide with the electrons in LEP in one intersection region. In the parasitic mode this yields luminosities of the order of 10^{29} cm^{-2}s^{-1}, and the electrons presumably will not be polarized. Special dedicated runs with ep might lead to luminosities of the order of 10^{32} cm^{-2}s^{-1}. However, since this would stop the entire High Energy Physics program at CERN except for one interaction region, it is not expected to be a prominent mode of operation.

It is clearly premature to speculate what the next step at CERN will be. However, it seems rather natural, as suggested by many people, to install a superconducting proton ring for 5 to 10 TeV protons depending on the status of superconducting magnet technology. This would result in a rather nice kinematical range for ep or $p\bar{p}$ collisions.

(2) DESY

(a) DORIS - The e^+e^- annihilation process can be studied between the mass of J/psi and the mass of the upsilon using DORIS. DORIS has an active experimental program based on complementary detectors. A large facility type detector, ARGUS, is now under construction and is expected to start data taking in late 1981.

A program to upgrade the luminosity in the single ring mode for high energy operation is underway. Higher luminosities will be reached by installing "mini-beta" sections (beta = 3 cm), an electrostatic plate system to separate the beams at injection and an improved sextupole correction system.

DESY, in collaboration with CERN and Karlsruhe, is engaged in a program to produce and test superconducting rf cavities. Karlsruhe has now produced a 500 Mhz cavity with an accelerating field of about 3.6 MV/m. The cavity is expected to be installed in the DORIS ring later this year.

(b) PETRA - PETRA has reached its design center-of-mass energy of 38 GeV. The peak luminosity at 35 GeV in center-of-mass energy is around 5 x 10^{30} cm^{-2} s^{-1} with an integrated luminosity of about 60-100 nb^{-1} a day. This should be compared to the predicted peak luminosity of about 2 x 10^{31} cm^{-2} s^{-1} for two bunches on two bunches colliding. Mini-beta sections will be installed in all four interaction regions. The installation will be completed towards the end of 1980 and is expected to increase the present luminosity by a factor of three-to-four.

PETRA has a full experimental program with five large detectors installed in the four experimental areas.

Installing conventional rf cavities in the two remaining long straight sections (leaving room for two new experiments) and doubling the available rf power from a 4 MW to 8 MW would enable PETRA to reach 45 GeV in the center-of-mass. Such a program, or part of it, will be carried out during the next two to three years. To increase the energy beyond 45 GeV requires superconducting rf cavities. It might then be possible to reach 60-70 GeV in center-of-mass energy.

(c) HERA - DESY has proposed to construct a large electron-proton colliding beam facility, HERA, designed to collide either electrons or positrons of 30 GeV nominal energy with protons up to 820 GeV yielding 314 GeV in the center-of-mass and a maximum momentum transfer squared of 98,400 GeV^2. This is equivalent to an electron beam of 52 TeV on a stationary target. The design luminosity of around 10^{32} cm^{-2} s^{-1} is sufficient to explore the region up to 40,000 GeV^2 with both charged and neutral currents. This machine has received strong support from the German high energy community and a project definition endorsed by ECFA has been forwarded to the German Government. A committee has been appointed to evaluate the merits of HERA and other large scientific projects and to establish a list of priorities. The committee is expected to report back to the Government in the fall of 1980.

A program to construct superconducting magnets based on the technology developed at BNL and Fermilab is underway. The construction time for the electron ring is estimated at 5 years and for the proton ring at 7 years.

It is in principle possible, at little extra cost to study e^+e^- collisions using the HERA electron ring. With the rf foreseen for ep operation such studies could be carried out at center-of-mass energies up to 70-75 GeV. Note, that in the standard model e^+e^-, annihilation is dominated by the Z^0-pole in this energy region.

SECTION IX

EVALUATION AND RECOMMENDATIONS

Evaluation

(1) Overall Program

The U.S. High Energy Physics effort, supported for more than 30 years by taxpayers of the U.S. through the Federal Government, has maintained over the years a diverse and exciting program of research and discovery. The DOE, as the main funding agency, supports the three large national accelerator Laboratories, as well as University research groups, while the NSF provides independent support for University groups and also the large, but still University-based facility, CESR. Existence of two funding sources gives the U.S. program an important dimension of flexibility and possibility for innovative alternatives.

The program has a broad spectrum of research with existing accelerators and storage rings at the three major Laboratories and Cornell: a 400 GeV fixed target program with proton-initiated neutrino, muon, photon, and hadron beams at Fermilab; 33 GeV electron-initiated photon and electron beams on fixed targets, and the e^+e^- storage rings SPEAR (3-8 GeV) and PEP (10-36 GeV), at SLAC; the 30 GeV proton-initiated fixed target program of neutrino and hadron physics with the AGS at Brookhaven; the 8-16 GeV e^+e^- storage ring CESR at Cornell. In the next four years, the Energy Saver/Tevatron project at Fermilab will add exciting new physics at TeV energies. By 1986, ISABELLE is expected to provide proton-proton interactions from 60 GeV to 800 GeV. The U.S. program will then be unmatched in the physics of proton-initiated interactions over the whole range up to 2 TeV center-of-mass energies.

The present program is very ambitious with a dependence on new superconducting technology having attendant high risks but very high benefits. Construction using the new large-scale technology of hundreds of superconducting magnets and their associated cryogenics, stretched over several kilometers of tunnel, is a major challenge. Existing budget levels are barely adequate to provide the necessary R&D to sustain orderly construction of both ISABELLE at BNL and the Energy Saver at Fermilab. Each Laboratory has different problems to overcome, with Fermilab further along. It has become clear in the doing that the difficulty of the necessary superconducting magnet technology was underestimated. The problems are greater and more numerous than anticipated. They are largely being solved, step by step, but it must be realized that these accelerators are devices more complicated by far than previous machines. The new facilities will require the concerted efforts of devoted scientists and engineers at both Laboratories to bring them to reliable operating status for physics. The successful completion of these facilities is the responsibility of the entire national community.

(2) Operations

With Fermilab involved in a major and highly technical construction project, the Energy Saver, the resources of the Laboratory are focused in such a way that the utilization of the existing 400 GeV accelerator for physics research is presently below 50 percent, the generally agreed-upon minimum acceptable level for a viable ongoing program. As the Energy Saver nears completion, further pressures on the 400 GeV program can be foreseen. Every effort should be made to sustain and even augment the 400 GeV running for physics while the Energy Saver is under construction.

Major efforts at the forefront of e^+e^- physics exist at Cornell's CESR and at SLAC with PEP just operational. These operating programs are devoted to exciting and important physics and must be supported vigorously in the coming years.

Fixed target programs exist at SLAC and BNL, as well as at Fermilab. A considerable diversity of physics at lower energies is encompassed by these efforts, although financial pressures have reduced the SLAC linac program to operation of the multiparticle spectrometer (LASS) and the rapid-cycling bubble chamber and have reduced physics running at the AGS to 25 weeks or less per year.

(3) Construction

For the immediate future at Fermilab, the building of the Energy Saver at 500 GeV, followed by the fixed target and $\bar{p}p$ collider at TeV energies, must have the highest priority. The energy-saving aspects of the Energy Saver are important in their own right, but the unique physics opportunities are what force the priority. A fixed target program at 1 TeV will have the highest energy neutrino, muon, photon, and secondary hadron beams in the world, and the $\bar{p}p$ collider will be a unique facility providing 2 TeV center-of-mass energy at moderate luminosity.

At BNL, the construction of ISABELLE must proceed as rapidly as possible. The physics goals of ISABELLE, utilizing proton-proton collisions at roughly 800 GeV cms energy with very high luminosity, are extremely exciting. Detailed study of charged intermediate vector boson production and decay is only one of the prizes awaiting ISABELLE. The present difficulties with magnet manufacture, a serious and unexpected setback in the light of earlier successful prototype superconducting magnets, can only be overcome by an orderly and vigorous R&D program.

(4) Research and Development

The continuing breadth of the field demands ongoing research and development in both advanced accelerator technology and detector development. Some of this R&D is project-specific, with amounts that are governed by relatively short-term requirements. Past experience shows that these needs have been seriously underestimated.

Long-range R&D funds must also be provided on a continuing basis. Past practice has been to allot roughly 1 percent of operating money to this area. The present difficulties with superconducting magnet technology show that economies of effort and funds and more expeditious construction can be realized in the future if more extensive R&D occurs early in the technological innovations necessary for new facilities. This can be accomplished at present funding levels by a slower pace, but momentum is lost and progress delayed by such a solution.

Long-range R&D should be supported at a level approaching 4 percent of the operating budget, as recommended by the HEPAP Subpanel on Accelerator R&D. In the area of detector R&D, it is important to have the involvement of University groups because of their different perspectives and, therefore, possibly different new ideas on the solutions of specific detection problems.

(5) University-Based Research

University-based research groups comprise the large majority of high energy physicists in the country. The vigor of their efforts reflects directly on the success of U.S. high energy physics. In recent years, they have suffered from loss of momentum caused by the reduced operation of existing accelerators, lack of adequate equipment funds, serious increases in necessary travel costs, and erosion of their efforts by inflation, as well as increased overhead charges. The health of the University groups is the health of the field. High Energy Physics will flourish in the future because of the graduate students who now enter the field through the Universities. Adequate support of these groups is essential.

(6) New Opportunities

In the past decade, immense progress in our understanding has come from experiments with electrons. The approximate scaling in deep inelastic scattering of leptons on nucleons, the new families of heavy particles uncovered in electron-positron storage rings, the hadronic (quark and gluon) jets found at higher energies, all demonstrate the power of electromagnetic interactions as an initiating mechanism. The present and near future program has electrons as a significant component, but at energies below 40 GeV. Because of the anticipated presence of the Z^0 at 80-100 GeV, with its associated cornucopia of physics, there is great interest and excitement in going to higher energies with e^+e^-. Similarly, the discoveries in lepton-hadron interactions point the way towards significant new advances in our knowledge of the fundamental interactions with the extremely large energy and momentum transfers available in electron-proton colliding beams.

The overall balance of the U.S. program clearly requires consideration of e^+e^- and ep capabilities at higher energies. Specific suggestions before the Subpanel for new facilities confirm this general view. It appears to be premature to make a decision now on the exact direction such efforts should take. Maturing initiatives can be considered within the next one or two years. Among these the Subpanel is pleased to acknowledge an emerging Canadian initiative for an electron ring at a proton laboratory.

The search for proton decay, predicted by unified theories, is currently underway in the non-accelerator part of the program. Within two or three years, we should learn whether or not the predictions are correct. A positive result will demand a vigorous exploitation of the new physics. The overall program must retain the flexibility to address such exciting opportunities.

In the longer range, there are other opportunities. Clearly, still higher energies will be important. Fermilab has a 5 TeV proton accelerator in mind. The prospects of particle beams at several TeV and of 10 TeV center-of-mass energy $\bar{p}p$ collisions are most attractive, but still remote in time, after the establishment of reliable large superconducting accelerators. SLAC is developing ideas for extremely high energy electron linacs and has discussed two large linacs that give 350 GeV electrons and positrons colliding head on. This intriguing idea evidently requires extensive long-range R&D before a firm conceptual design can emerge. Other projects, perhaps very different from either of these will surely emerge as the field continues and benefits from experience with the new facilities.

Recommendations

The total High Energy Physics program involves large and complex construction projects at two accelerator Laboratories and just completed projects at the others, with underutilization of existing accelerators and warning signs of trouble if present trends continue. The Subpanel considered carefully the implementation of the ongoing program within the constraints of the present constant effort plan (DOE/OMB Long Range Plan) and also within a plan calling for a 15 percent increase of effort beginning in FY 1982. The recommendations that follow are within the framework of the DOE/OMB Plan. Following those recommendations are some recommendations assuming a 15 percent increase. Finally, a specific recommendation is made on the SLC, the only new construction proposal before the Subpanel.

Recommendations and Comments (base level/constant effort)

Recommendation 1:

The Fermilab 400 GeV accelerator, the newly commissioned PEP storage ring, and the CESR facility at Cornell must be utilized as fully as possible to exploit for physics the large investments already made.

Recommendation 2:

Construction of the Energy Saver and of ISABELLE must proceed with all deliberate speed. Necessary R&D funds must be provided to assure their success.

Recommendation 3:

The Tevatron I and II projects at Fermilab should proceed together. This requires authorization and initiation of Tevatron II in FY 1982.

Recommendation 4:

Options for the future must be kept open. Of great importance are the research and development of the technologies of very high charge-density bunches with low emittance in linear accelerators, very high-field superconducting magnets, and superconducting rf cavities.

Recommendation 5:

University-based groups should receive increased support to assure vitality of their efforts on immediate experimentation and also on detector development for the future.

Recommendation 6:

Accelerator studies and technical research should begin immediately toward the goal of starting the construction of a very large accelerator during the second half of this decade.

Recommendation 7:

A Woods Hole Subpanel or equivalent should be convened in the future to assess the status of construction, of new developments and of new opportunities, probably in 1981 and certainly by 1982.

Comments on the Recommendations:

(1) The constant effort DOE/OMB Plan, at its present level, is very tight, particularly with the two ongoing construction projects. Operations, especially the Fermilab 400 GeV program, cannot be permitted to decline further. CESR and PEP, the front-line U.S. efforts in e^+e^- interactions, must be supported strongly. Exciting physics remains to be done at SPEAR. Inevitably, the program, with a very constrained budget overall, cannot afford to support all components equally. The lower energy, fixed target programs at the AGS and SLAC linac have lower priority. Nevertheless, these programs contain a number of very important experiments providing opportunities for considerable numbers of University users.

(2) As discussed in Parts 1 and 3 of this Section, the development and implementation of superconducting magnet technology at Fermilab and BNL is a major challenge. The Energy Saver will be the first large accelerator with superconducting magnets. It is extremely important to have this project completed as soon as possible to prove out the very complex technology. Adequate R&D funds must be provided to meet exigencies. The same applies to ISABELLE at BNL.

(3) Under the DOE/OMB Plan, beyond Tevatron II there is simply no money for new construction to start in FY 1982. This fixed target program, which will be unique in the world, has overriding priority for construction funds. The facilities at Fermilab, after completion of the Energy Saver-Tevatron I-Tevatron II project, will support an extremely strong and diverse research effort in High Energy Physics.

After significant progress toward completion of the large-scale construction at Fermilab and BNL, new initiatives can be contemplated even under the plan of constant effort. The subsequent Subpanels (Recommendation 7) will confront such issues.

(4) Even though the funding of new construction projects beyond the Tevatron must be deferred at this time, long-range R&D on advanced accelerator technology should be pressed vigorously. Power consumption alone is a compelling argument for the work on superconducting magnets and cavities. Successes in one or more areas would be immensely valuable in expediting U.S. efforts towards facilities of the future.

(5) The needs of the University-based groups are discussed in Part 5 above.

(6) The Subpanel knows of no fundamental impediments to the achievement of electron energies of several hundred GeV and proton energies of 10 TeV or more. The Subpanel is convinced that the scientific value of accelerators in such energy ranges is extremely high.

Accelerators that provide beams of such energies and collisions between such beams are certain to be expensive to build and to operate. Research to minimize costs and to exploit new avenues will be long and difficult. Sustained efforts should be started now if we are to be ready to start such a new project during the second half of this decade.

(7) Now is a difficult time for decisions. On the one hand, we have huge construction efforts with difficulties beyond the normal range. On the other hand, there are exciting physics opportunities involving electrons, but with uncertainties as to the specific facility to implement them. Substantial progress on the advanced accelerator developments should be achieved within the next year or two. The community will then be in a better position to decide on the future thrust of the field.

Modified Recommendations (15 percent increase in effort)

A 15 percent increase in the support level beginning in FY 1982 would make possible major improvements in the program. First of all, the underutilization of existing accelerator facilities could be redressed. The support of research groups, particularly in the Universities, could be made commensurate with the needs of a preeminent scientific program.

Even more important, there would also be an opportunity for new construction initiatives beginning in the first half of the 1980's decade. One very exciting possibility would be an e^+e^- collider operating in the energy range near 50 GeV x 50 GeV. Such a machine would be capable of producing the neutral vector boson (Z^0) in abundance, allowing a rich program to study its decay modes and restoring more balance to the future U.S. program which is currently aimed primarily toward hadron machines. As an example, the proposed SLAC Linear Collider (SLC) might provide one way of doing this physics at a cost which could be acommodated within the early part of the decade in the upper level budget. Alternatively, the Cornell group has proposed a superconducting rf cavity R&D effort aimed toward the construction of a 50 GeV x 50 GeV circular colliding e^+e^- machine. If the R&D effort were successful, construction of this machine might be initiated around FY 1984 and completed a few years later. It is worth noting that a machine with appropriately optimized luminosity in the center-of-mass energy range between the PEP upper limit and the Z^0 mass might very successfully compete with the large European e^+e^- facility LEP. Yet another attractive possibility would be an ep colliding beam facility coupled to either ISABELLE or the Fermilab Tevatron. Possible ep facilities, being explored independently by a U.S. (Nevis Laboratory) group and by a Canadian group, were described to the Subpanel. Either would involve a 10 GeV electron ring, used in conjunction with a high energy proton beam at Fermilab.

It is very clear that the scientific balance and productivity of the U.S. High Energy Physics program would be greatly enhanced by the addition of a new facility dedicated to the physics objectives outlined in the e^+e^- and ep submissions made to this Subpanel.

RECOMMENDATION AND COMMENT ON THE SLC PROPSOAL

Recommendation

The Subpanel recommends that no decision be taken on the SLAC SLC proposal at this time.

Comment on the recommendation

The Subpanel studied the proposal to construct a linear collider system at SLAC starting in FY 1982. This is an innovative, detailed proposal that addresses a very important physics goal, the existence and properties of the neutral vector boson, Z^o. Furthermore it addresses research and development questions important to the extension of e^+e^- experimentation to very high energies.

Even though there are major new features in this machine (notably the transmission of small, intense bunches of electrons and positrons and a new regime of beam-beam interactions), the technical subgroup found no reason to doubt the claim that a luminosity in one interaction region of 10^{30} cm^{-2} sec^{-1} can be achieved within the proposed time scale and budget. Particularly attractive aspects of the proposed collider are the relatively low cost, the imaginative use of much well known technology, the realistic possibility of polarized beams; and, finally, its role as a demonstration project for possible very high energy linear colliders.

For the base funding level, there are simply no funds available for new initiatives at this time. For the higher funding level, the Subpanel recommends delaying a decision on the SLC, in spite of the very positive features just described, for the following reasons.

The SLC would most likely compete with the CERN LEP project for the physics of the Z^o. To be in a strong competitive position, SLC or any other U.S. e^+e^- initiative should have a high probability of achieving in a timely fashion luminosity comparable to or in excess of that of LEP in the Z^o energy region. Exploitation of some of the interesting physics at the Z^o may require luminosities in excess of 10^{31} cm^{-2} sec^{-1}. This is true, in spite of the relatively large total e^+e^- cross section, because the branching ratios for many interesting processes are expected to be very small. The proposal states a design luminosity of 10^{30} cm^{-2} sec^{-1}. The proponents, however, believe this to be conservative and discussed a number of factors which could raise the luminosity to 10^{31} cm^{-2} sec^{-1}; but detailed analysis was not provided.

As discussed elsewhere, there is great interest in e^+e^- physics between the high energy end of PEP/PETRA and the Z^0 region. The SLC may or may not be the optimal machine for this energy range. Yet, its approval now would naturally preempt another possibly more useful instrument. An example of an alternative machine has been described in a proposal for R&D funds by the Cornell group. There is a whole complex of advantages and disadvantages to each machine bearing on issues of cost, time scale, luminosity, number of interaction regions, energy spread, beam polarization, and relevance to future directions. In the process of making a decision on the SLC, it is important to consider carefully serious alternatives, such as the possibility of construction of a facility providing both e^+e^- and ep collisions. This should be done in a timely fashion. Nonetheless, a modest delay in a decision on the SLC project has the benefit of permitting a more complete airing of the technical issues and a wider involvement by the U.S. High Energy Physics community.

It will be appropriate to review this situation in one to two years as more information both on technical issues and future funding levels becomes available.

APPENDIX A

Charge to Subpanel

Within the context of changing worldwide high energy physics activities and opportunities, review of the status and prospects of the U.S. program, taking into consideration all aspects of the program, including:

- o Physics progress and achievement of scientific understanding.
- o Physics research programs at Universities and Laboratories.
- o Laboratory facility operations (including program scope, operating effectiveness, utilization levels and user research opportunities).
- o Technology R&D for accelerators and experimental facilities.
- o Progress on facilities recently completed or under construction.
- o Future construction proposals and possibilities.

Develop a general strategy and long-range plan for the U.S. program over the next decade and, in particular, assess the program balance required for physics research, facility operations, technology development, equipment and construction over the period from FY 1982 to FY 1987, and make specific recommendations for FY 1982; all under the following funding constraints:

1. The same guidance as that under which the program now operates; i.e., an average annual funding level for the U.S. program (DOE plus NSF) of $325M in FY 1979 dollars.
2. A funding constraint 10-15% higher than that above.
3. Are there important high energy physics activities which cannot be conducted on a timely basis within the above constraints? What are they are what would be the implications of adding or excluding them?

In an exploratory field such as high energy physics, major scientific achievement rests on the capacity to carry out research at the frontiers of knowledge and technology. Within each of the above funding constraints, assess the prospects and sufficiency of the strategy and long range plan to maintain a forefront U.S. high energy physics program.

APPENDIX B

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1980 Subpanel on Review and Planning

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THE U. S. DEPARTMENT OF ENERGY HIGH ENERGY PHYSICS ADVISORY PANEL REPORT OF THE SUBPANEL ON ACCELERATOR RESEARCH AND DEVELOPMENT

JUNE 1980

STANFORD UNIVERSITY

FORD LINEAR ACCELERATOR CENTER

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June 4, 1980

Dr. Edward A. Frieman, Director
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Washington, D. C. 20545

Dear Ed:

I am pleased to transmit to you with unanimous endorsement by the High Energy Physics Advisory Panel this report on "Accelerator Research and Development in the United States High Energy Physics Program." This report, prepared by the HEPAP Subpanel on Accelerator R & D under the Chairmanship of Professor M. Tigner, is a strong and persuasive call for the U. S. to put considerably more emphasis on long range accelerator R & D.

We are currently devoting little more than 1% of the high energy physics budget to advanced accelerator R & D which focuses on technologies looking five or more years into the future. The central recommendation of this report is that this is inadequate, and that we should be prepared to increase this level substantially as the opportunities arise. A level of effort constituting up to 4% of the U. S. program, as advocated in this report, has our full endorsement.

This report is both an encyclopedia of very great value, of the ongoing efforts and of novel acceleration schemes under consideration, and a "call to arms" encouraging more attention on the part of the high energy community to advanced accelerator R & D. As we see the end of the line for today's technology in advancing the high energy frontiers, it is of great importance that new techniques be explored and developed for the future. New accelerator techniques and the associated R & D programs will very likely also be of value to other fields of interest to the Department of Energy and the country.

I hope this excellent report will receive study and will be broadly distributed. I urge that its recommendations be used to guide policy for the accelerator R & D effort in our national high energy program.

Sincerely yours,

Sid

Sidney D. Drell
Chairman, HEPAP

dp
enclosure

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August 26, 1980

Prof. S. Drell
Chairman, HEPAP

Dear Sid:

Herewith I am transmitting the report of the HEPAP Subpanel on Accelerator R/D.

You will note that in the 50 odd years of American Accelerator science associated with particle physics research, enormous strides in increasing particle beam energies and in decreasing unit costs have been and are being made. Our report gives a catalog of high energy accelerator R/D now in progress.

Our primary conclusion is that, despite the spectacular past and present accomplishments of the field, we must redouble our efforts to improve the cost effectiveness of our accelerators if the needs of US particle physics are to be met in the resource limited situation in which we find ourselves. This needed improvement will entail continual and vigorous improvement of existing technologies as well as the discovery and reduction to practice of new acceleration methods and technologies. If we are to meet this challenge we will need to devote more of our intellectual and monetary resources to high energy accelerator R/D than is now our practice.

Yours very sincerely,

Maury Tigner for the
HEPAP Subpanel on Accelerator R/D

MT/mn

Members of the Subpanel
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EXECUTIVE SUMMARY

Accelerator Research and Development has always been an integral and important part of elementary particle physics research. The physics output has been paced, to a large extent, by the improvement in performance of our accelerators. In the past 50 years, equivalent beam energies available to experimenters have increased by a factor of ten million as a result of accelerator R&D efforts. Not only has performance increased exponentially but unit costs have also been reduced significantly. Even in times of decreased real spending power for the field, significant energy increases were achieved.

Despite our remarkable achievements in unit cost reductions, these reductions have not kept pace with the energy increases achieved. This has resulted in a limitation in the number and diversity of high energy accelerator facilities available to workers in the field. Further decreases in diversity would be necessary to continue frontier research were the pace of cost reduction produced by new accelerator technologies not to increase. This would have a serious negative impact on the scientific productivity of U.S. elementary particle physics. The challenge is clear: we must redouble our efforts to reduce unit costs sharply while maintaining performance improvements through full exploitation of the potential of our current accelerator technologies and through invention of entirely new technologies and methods.

Historically, we have spent approximately 10 percent of our operating resources on accelerator R&D activities. This includes both R&D applied to projects (RDAP) and long-range R&D (AARD) which is not associated with a specific project. Today this fraction for all accelerator R&D is 14%, reflecting our heavy commitment to superconducting magnet development. The great majority of this is RDAP, as it needs to be. The Subpanel has determined that between 1% and 1 1/2% of operating resources are now devoted to AARD. If we are to meet the challenge of the future, more of our resources need to be devoted to this activity. We believe that an appropriate level for long range R&D would be about 4% of HEP operating resources.

It is obvious that no level of expenditure for AARD can guarantee that the historical trend in unit cost reduction can continue. Nevertheless, the Subpanel is convinced that without increased effort on AARD significant unit cost reduction is most unlikely. Thus, we conclude that increased effort on AARD is an essential investment for the community at this time.

Recommendation

1. The high energy physics community should significantly increase its support for AARD. The Subpanel estimates, from an examination of significant areas of this research, that the level for AARD in the HEP accelerator field should be increased from about 1% up to about 4% of current operating resources. This increase should be achieved within the existing administrative framework.

2. In reviewing accelerator R&D, the Subpanel has identified certain specific technical areas that should be emphasized in AARD. This list is not presumed to be complete, exclusive, or to indicate relative priorities. Rather, it is intended to be an indication of the challenges of AARD and to underscore the importance of AARD to the field of high energy physics. This list of specific topics is:

   (a) Development of very high field accelerator magnets and the evaluation of the practical limits of this technology. In view of the large-scale of this enterprise and its uniqueness, the Subpanel recommends that this AARD effort be carried out as a collaborative effort among the laboratories having capability in this area. The Subpanel feels that this development of high-field magnets should be focused toward a specific accelerator goal.

   (b) Development of liquid helium refrigerator systems with goals of improving efficiency and reliability and providing operation at reduced temperatures.

   (c) Theoretical and experimental exploration of the limits to microwave linac gradients and to the peak powers that can be delivered in the S- to X-band regions.

   (d) Basic physics and device development in superconducting RF accelerators.

   (e) Theoretical and experimental studies of basic accelerator phenomena, particularly the beam-beam interaction and other performance-limiting phenomena.

   (f) Search for and preliminary development of new accelerator schemes with high performance potential such as laser accelerators or other devices using ultra-high peak power with or without collective efforts.

   (g) New techniques and devices for manipulating very high power and/or very high energy beams.

   (h) The general problem of increasing the brightness of particle beams with emphasis on cooling high energy beams.

   (i) Development of new beam diagnostic techniques and devices.

3. Laboratory and University managements, aided by the agencies, should take specific measures to make participation in accelerator R&D practical and more desirable for high energy physics theorists and experimentalists not now so engaged. This could include payments of salary for sabbatical leaves and leaves of absence, and on-the-job training through seminars, summer schools and the use of specially assembled tutorial and review materials. Also included could be expansion of university-laboratory collaborations.

4. Laboratory and University managements, aided by the agencies, should take specific measures to facilitate cross-fertilization from other fields such as Plasma Physics, Lasers and Materials Science, and other accelerator-relaced activities. This could include long-term visits by outstanding individuals in these fields for the purpose of working directly with labortory staffs, giving tutorial seminars or lectures series, or participating in extended topical workshops.

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I. INTRODUCTION

In this report we review accelerator R&D in the U.S. High Energy Physics (HEP) program. As a result of this study, we conclude that some shift in priority, particularly as regards long-range accelerator R&D, is required to best serve the future needs of the U.S. HEP program. We also suggest some specific new directions for the U.S. R&D effort.

Throughout this report, we shall use the term "accelerators" to denote not only devices which accelerate particles but also storage rings, colliders, etc.

Our assessments and recommendations are strongly influenced by the fifty year history of the field. Moreover, the particle energies needed to test the validity of current theoretical ideas reinforce our conviction that the historical thrust to higher energies must be maintained. Finally, we are persuaded that, as in the past, new and unsuspected phenomena will be uncovered at higher energies. These considerations lead us to the view that for the foreseeable future this thrust to higher energy must be maintained via a diversity of approaches: diversity of beam particles, accelerators, and colliders, as well as unorthodox approaches.

These needs have important economic consequences which present a direct and difficult challenge to the national accelerator R&D program. Great ingenuity and resourcefulness will be required to meet this challenge.

A. Historical Trends as a Guide to Future Needs

The remarkable evolution of accelerator beam energies over the past five decades is partially displayed in the "Livingston Chart" of Figure 1. Beam energies (or fixed-target equivalent beam energies in the case of colliding beam accelerators) have risen by seven orders of magnitude in the past fifty years. It is noteworthy that this dramatic increase in collision energy has been achieved by a succession of diverse technologies: as any one technology reached saturation, it was superseded by a new, more powerful method based on novel ideas. This ~~trend emphas~~izes the crucial importance of advanced accelerator R&D, which is the source of such new approaches.

Much, but by no means all, progress in the understanding of elementary particle physics can be ascribed to increasing accelerator energies. In the 1930's, elementary particle science had already established that the atom was a composite structure involving pointlike electrons bound electromagnetically to a heavy nucleus. The nucleus is itself a bound composite structure of particles, now recognized as protons and neutrons, or collectively as nucleons. The binding together of the nucleons in the nucleus is much strong than that of electrons to the nucleus. Disintegration of nuclei to study their structure therefore required projectile energies of several millions of electron volts (MeV) rather than the several thousands of electron volts (KeV) required to elucidate the electronic structure of the heavier atoms. As our ability to accelerate particles improved, we were able to see that the nucleons in turn are very

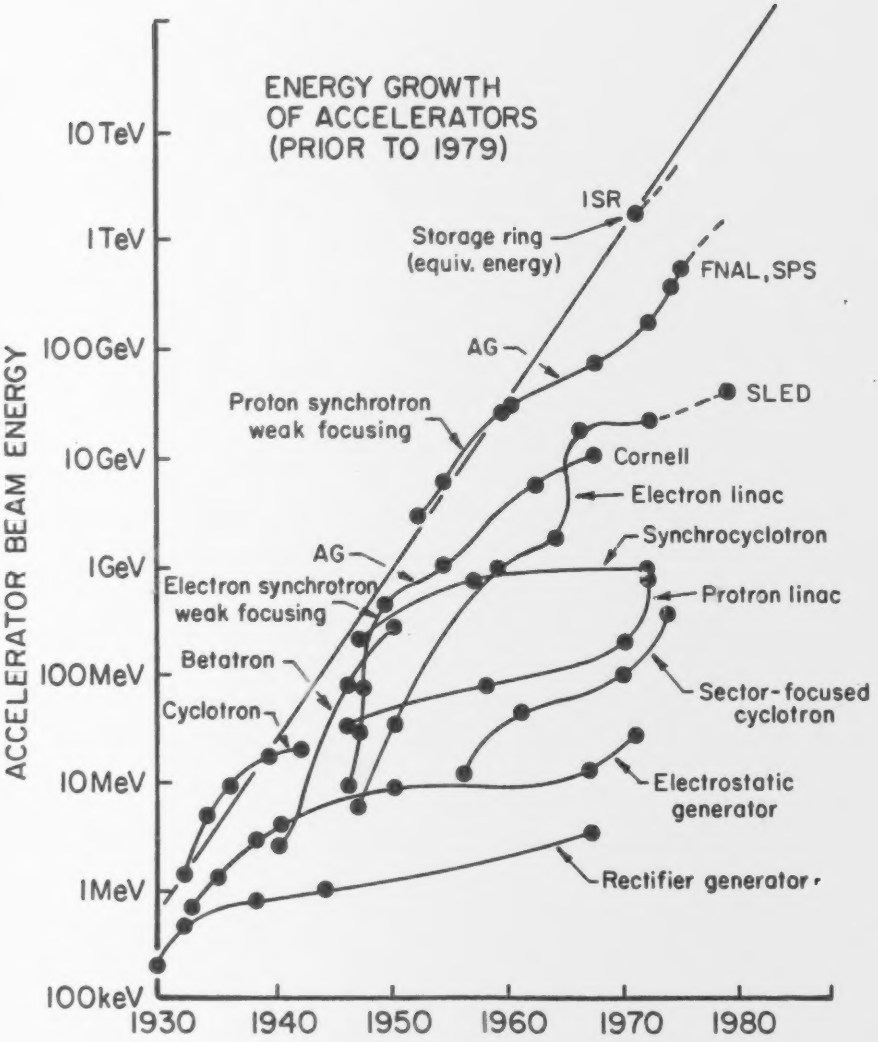


Fig. 1 Livingston Chart

tightly bound composite particles with a rich substructure, the investigation of which has required projectiles of much higher energy. At the present time, available beam energies have reached into the hundreds of billions of electron volts (GeV). From experiments performed at these energies, a reasonably self-consistent picture of matter has emerged. Fundamental constituents of matter now appear to be pointlike leptons, of which the electron is one, and quarks which are new types of particles apparently confined in the nucleons.

Historically, as each new layer of matter was uncovered, a new "spectroscopy" was found which revealed the existence of smaller, more fundamental particles. Increasing volumes of data then led to a better understanding of the basic forces in nature. Yet as the basic richness of nature, on an ever-decreasing scale of distances, became apparent, so did its underlying simplicity: the laws of electricity and quantum mechanics were found to remain valid over an amazingly large range of distances. Moreover, during the past decade, experiments have shown the road to a basic unification of the diversity of forces in nature. It seems likely that the history of the past will repeat, and that higher energies will reveal further richness and new perspective in this fundamental picture. Indeed, the current theoretical understanding in the field provides a basis for specific expectations concerning new phenomena awaiting exploration with future machines at higher energies.

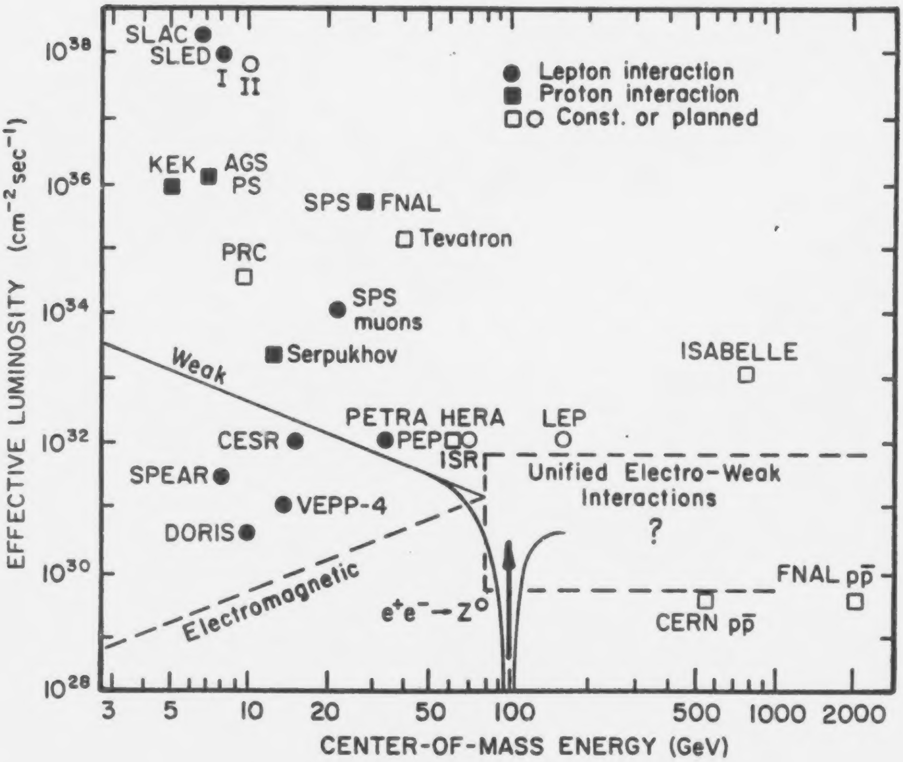
While these machines must clearly have sufficient energy to be above the threshold required for new effects, energy alone is not sufficient. We must also produce an intensity or "luminosity" which will allow an observable rate for those fundamental processes which occur only very rarely. The event rate is given by the luminosity multiplied by the cross section (a quantity which measures the probability) for the reaction of interest. Figure 2 plots the collision energies and luminosities of several existing and planned accelerators and colliding beam devices. Also in the future are lines showing the luminosities required to yield sufficient intensity to permit investigation of all the various forces in nature. Such considerations provide an additional challenge to accelerator R&D, the high energy machines of the future will only be useful if they yield adequate luminosity.

B. Physics Expectations at Higher Energy

In predicting the future utility of accelerators and colliding beam devices for research one has to be mindful of several basic factors: (1) expected thresholds, (2) conjectured phenomena at ultra-high energies, and (3) "the unknown."

1. Expected Thresholds

Of all the forces in nature, electromagnetism is the best understood. The unifying theory describing it, Quantum Electrodynamics (QED), has withstood experimental tests at the highest energies available. The electromagnetic force couples electric charges through the exchange



Effective luminosity vs. center-of-mass energy for the largest accelerators and storage rings now operating, in construction or planned. For accelerators, the target is assumed to be one meter of liquid hydrogen, except in the case of "SPS muons," where 50 meters of LH_2 is assumed.

The curves indicate the luminosities required to achieve a counting rate of one event per hour (assuming 100% detection efficiency) for the electromagnetic and weak interactions. The resonant dip in the weak-interaction curve applies if the neutral-current weak interaction is mediated by a Z° boson of mass 100 GeV.

Fig. 2

of massless vector particles called photons. Somewhat less understood is the weak interaction, which is believed to couple the combinations of flavors of both leptons and quarks through exchange of massive intermediate vector bosons. Finally, on a less detailed level of understanding, we believe the strongly interacting particles to be composed of quarks, which are thought to be confined within such particles (hadrons) by massless vector particles called gluons. These gluons couple to the quark property called color. The theory describing these interactions of quarks and gluons is called Quantum Chromodynamics (QCD).

This current picture, which is summarized in Table I, is expected to remain correct in most essential aspects but at present retains many unknowns.

The families of leptons and quarks which are known today are shown in Table II. Note that there is a missing quark (the t) whose mass, if it exists, may be above the reach of current machines. Note also that the discovery of these fermions originates from a large variety of sources. There never has been, and we surmise there never will be, a single "best" kind of machine for discovery. Only further experiments, most requiring higher collision energy, are apt to clarify, modify and supplement this picture.

In accordance with this current description, collisions involving either electrons and positrons or nucleons and (anti) nucleons are described in terms of elementary lepton-lepton collisions or in terms of combinations of quark-quark or gluon-gluon collisions, respectively. The reason is that hadron beams are a mixed collection of quarks, antiquarks and gluons with a wide energy spread, while lepton beams are accompanied by a small number of photons and intermediate vector bosons, increasing in numbers with energy. Table III roughly illustrates the situation for e^+e^- and $\bar{p}p$ collisions.

According to this general picture it is expected that to explore adequately the threshold for a new phenomenon involving the creation of a new object of mass M, then the combined beam energies of mutually annihilating electron and positron beams should be, say, 10-20% above the value of that mass, while for proton colliding beam devices the combined beam energies might need to be as high as, say, roughly three times the value of the new mass. Happily, proton machines continue to be less expensive for a given beam energy than electron machines and thus compensate for this difference in thresholds which is caused by the larger number of degrees of freedom in nucleon-nucleon collisions. It also follows that to explore a given kind of phenomenon the required luminosity for electron-positron machines should be roughly two orders of magnitude larger than that for proton-antiproton devices. The reason for this is simply the disparate magnitudes of the structure constant of electrodynamics (1/137) and the coupling constant of QCD (1/6).

The Present Picture of the Interactions of Elementary Particles

	Electromagnetic	Weak	Strong
Couples with	Electric charge	flavor	color
Carried by fermions	quarks and leptons	quarks and leptons	quarks
Couples through exchanoe of	photons (neutral, massless)	intermediate vector bosons (neutral & charged, massive)	gluons (colored & massless)

Table I

	LEPTONS		QUARKS		
ELECTRIC CHARGE	-1	0	$-\frac{1}{3}$	$+\frac{2}{3}$	DATES & NOTES
	e^-	ν_e	d	u	$\leq$1935 Basic building blocks of all chemical elements
	μ^-	ν_μ	s	c	1936 Muon (μ) discovered in cosmic rays 1947 Strange particles 1974-76 ψ/J, Charm
	τ^-	ν_τ	b	t(?)	1975-77 τ discovered in e-μ events 1977 Upsilon (Υ) discovered

Table II - Three generations of leptons and quarks

Comparison of e^+e^- and $p\bar{p}$ collisions

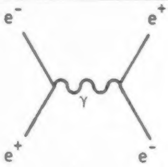
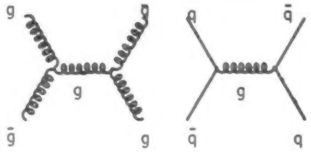
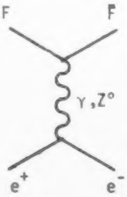
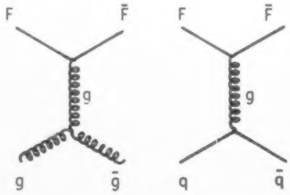
e^+e^- Collisions	$p\bar{p}$ Collisions
e^+e^- makes new charged quanta through photons or intermediate vector bosons	$q\bar{q}$ or $g\bar{g}$ make new colored quanta
Bhabha scattering  Strength: $\alpha^2 = (1/137)^2$	$g\bar{g}$ $q\bar{q}$ hard collisions  g g g $\bar{g}$ g q $\bar{q}$ g $\bar{q}$ q Strength: $\alpha_s^2 \sim (1/6)^2$
Annihilation into charged fermions F  F $\bar{F}$ γ, Z° e^+ e^-	Annihilation into colored fermions F  F $\bar{F}$ g g $\bar{g}$ F $\bar{F}$ g q $\bar{q}$
Threshold: $m_F \lesssim 0.8\ E_{beam}$	Threshold: $m_F \lesssim 0.8\ E_{g,q}$ $\lesssim 0.4\ E_{proton}$

Table III.

One must, of course, take account of the fact that in electron-positron collisions we are starting from a cleaner initial state and that therefore the interpretability of the final event will generally be enhanced. Also, electrons and positrons open the possibility of direct channel resonances leading, for instance, to copious production of such objects as the Z^0 or other new particles. Some new discoveries will therefore be made easier by the extension of electron-positron energies. However, such considerations will ultimately be dwarfed by the technological opportunities to reach very high energies to start with, and these technological opportunities look most promising for protons. In view of the above, electron-positron and proton-(anti) proton collisions offer alternative paths to particle discoveries at highest energies: electron-positron collisions can lead to discoveries less masked by background, while proton collisions will ultimately reach higher threshold energies and offer the possibility to explore channels not accessible through electron-positron collisions.

According to the above theoretical picture, the next families of fundamental objects that might be uncovered are the intermediate vector bosons in their neutral and charged varieties. This will require collision energies approaching 100 GeV for electron-positron collisions and several times that for hadron collisions. These particles should be produced by machines to be built in the late 1980's. It is expected that production of the Z^0 should be sufficiently copious that electron-positron colliders will be "factories" of such objects.

2. Conjectured Phenomena

According to current thought, the above picture cannot be complete and may well be defective. Since the vector carriers of the weak interactions have finite mass, they cannot be purely transverse in character as is the photon and its electromagnetic field, there should also exist scalar particles related to the longitudinal components. While specific predictions of the nature and masses of these "Higgs" particles remain uncertain, they might constitute a whole new family of objects, and these in turn might give rise to a new spectroscopy.

We also do not know whether the entries in Table II (assuming that the t quark will be discovered) are complete. There could be further generations of leptons and quarks at higher energies, and their discovery would have profound implications for both basic particle theory and cosmology. If the t quark does not exist, considerable re-thinking and rearrangement of the quark pattern will be needed.

Beyond the lepton and quark generations described above, there might be entire new families of fermions which do not interact through the color forces, like quarks, but which have carriers of a new type. Some of this might be visible in the 1 TeV center-of-mass energy regions. Moreover, there is an excellent rationale for going beyond that. For one thing, one must recognize that the variation of basic cross sections with energy predicted by QCD is largely logarithmic, and that therefore considerable increments in collision energies are necessary in order to pin down such basic dependencies reliably.

There are indications from cosmic-ray experiments that at beam momenta against fixed targets in excess of, perhaps, 10^{14} eV/c new phenomena come into play, which seem to give rise to jets of very high multiplicity of charged particles but apparently free from neutral pions. These phenomena, to be seen at all in cosmic rays, must have cross sections of sufficient magnitude to make them accessible to future colliders even at luminosities well below those attained in the past. Well above even these energies, there is the conjecture that some extremely heavy objects exist in nature which become dominant once the grand unification of electromagnetic, weak and strong interactions has taken place. These particles have masses well in excess of those conceivably attainable by man-made accelerators in the foreseeable future.

Thus it is indeed unlikely that the quest for higher and higher energies will become unproductive, i.e., will lead to a "flat" world of the energy dependence of elementary phenomena. Rather, the manifestation of such high masses will impact observable phenomena progressively more significantly as collision energies become higher.

3. "The Unknown"

This discussion is, of course, based on tentative reasoning. For each new energy region that becomes accessible, searches for "unknown" phenomena will be made. Some of the most fundamental questions in physics, which go beyond the question of completeness of the entries in the above tables, remain unanswered. For example, searches for magnetic monopoles and free quarks will be continued with higher energies. We do not know why the positive and negative objects in the tables appear in pairs -- this might possibly break down at higher energies. We also do not know today whether leptons and quarks will remain "pointlike" at distances smaller than 10^{-16} cm. Nor do we know whether quarks and leptons are bound states themselves. These questions lead to a need for experiments involving larger transfers of momentum, in turn requiring higher energy collisions than those accessible today and in the eighties.

Free quarks have not been observed, though the indirect evidence for their presence inside hadrons is clear. The mechanism of the confinement is a very active subject of research. One possible mechanism suggests that these may be ne states which correspond to voids of "bubbles" in the vacuum. It is possible that electron or proton collisions are not the best way to create such states, but that collisions between very high energy nuclei would be more effective in forming these states or other unusual states containing the presently known quarks.

Any such forecast must be approached with some humility. Historically, theoretical predictions for future phenomena in physics have been fallible, in pointing out the success of some theoretical predictions one should not lose sight of past failures. In fact, experience has shown that rarely have accelerators and colliding beam devices been built for the right reason! Although the specific expectations put forward by the proponents of new devices have generally been fulfilled, the most important impact of accelerating and colliding beam devices has generally been in areas not originally foreseen by the proponents.

C. Economic Consequences and Conclusions

Over the fifty year history of the field, unit costs measured in terms of \$ per equivalent beam energy have decreased by approximately a factor of a million. This impressive record has been achieved by a series of past developments of new techniques. Still, modern accelerators have become more expensive. In an era of constant purchasing power available to the field, future advances in energy will have to be achieved with further reductions in unit cost, such that overall accelerator costs do not increase substantially. This clearly represents an enormous challenge to the accelerator community, and will require considerable R&D over the coming years on a variety of techniques. Figure 3 shows a projection of the "Livingston Chart" beyond 1980 for both proton and electron machines.

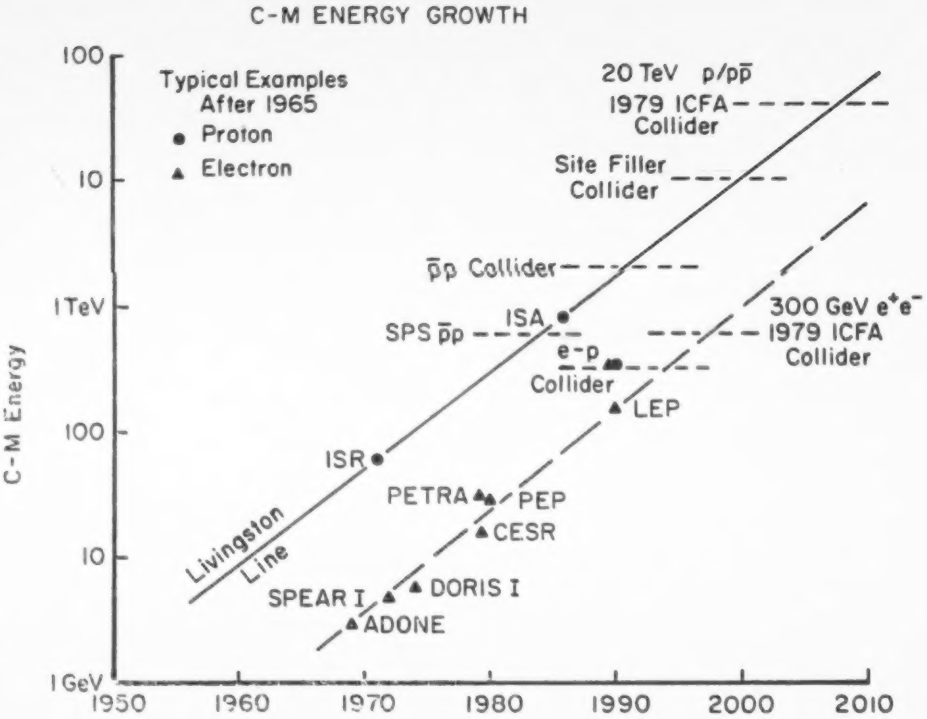


Fig. 3 "Projection of the Livingston Chart Beyond 1980 for both Proton and Electron Colliders"

II. CHARGE TO THE HEPAP SUBPANEL ON ACCELERATOR R&D AND SCOPE OF REPORT

A. Charge

The HEPAP Subpanel on Accelerator R&D is to review the overall scope and quality of the accelerator R&D effort in the U.S. High Energy Physics program. This is to include:

1. An examination of the existing accelerator and detector R&D effort in the U.S.

2. A comparison of the U.S. effort with efforts abroad.

3. Specific recommendations concerning the U.S. accelerator R&D effort, with particular emphasis on:

   (a) breadth and depth of the R&D effort;

   (b) balance among short-term, mid-term and long-term R&D,

   (c) priorities within the U.S. accelerator R&D effort,

   (d) appropriate funding levels.

This is to be a broad review. In this sense a central question is whether the planned accelerator R&D level of effort and content will provide an adequate scientific and technological base for improvement of present accelerators and design of future accelerators that may be needed by the HEP research community. It is assumed that some multiplicity of technological options must be kept open. What accelerator R&D effort is required to do this? A general approach is needed, and not a specific concern about potential construction projects.

The Subpanel is requested to consider detector R&D and potential new detectors in addition to accelerator R&D, because of the strong influence that advances in detector technology can have on requisite accelerator and storage ring performance. In this respect the Subpanel is being asked to put their projections for accelerator R&D within the context of a forecast of future detector capabilities and their implications for long-range accelerator R&D and not to conduct a broad review of detector technology.

An initial report will be made to the Chairman of HEPAP prior to the initial formulation of the FY 1982 budget which will begin about March 1980. A final report should follow soon after.

B. Comments on the Charge

After convening, the Subpanel discussed its charge. It was felt necessary, given the short time available, to limit the scope of the review. It was unanimously felt that it was most important to focus on questions connected with the long-term future of the field. Accordingly, it was agreed to devote the bulk of the Subpanel's detailed considerations and recommendations to the long-range component of accelerator R&D. R&D in support of current accelerator projects and detector R&D were covered only briefly. This decision was conveyed to HEPAP and DOE.

C. The Relationship of Accelerator R&D to Detector R&D

Progress in high energy physics research clearly requires ongoing research and development in the accelerator, detector and data processing areas. While it is true that these fields are interdependent and that developments in one affect developments in another, the pace of accelerator R&D can be usefully considered independently of questions concerning detector and data processing R&D.

The fundamental reason for this point of view is the fact that the leading edge of our field is accelerator limited for the foreseeable future. This is not to say that detector R&D is not needed -- indeed it is if we are to exploit fully our present and future accelerators. Nor is it to deny that there are some fundamental aspects of high energy research that can only be addressed with new and improved detectors. Nor do we disagree that modern digital data processing techniques are crucial to the entire field of high energy physics. However, it is our opinion that any foreseeable accelerator development would be capable of significant utilization with the detector art we have, or can reasonably expect to have. On the other hand, without new accelerator developments, significant regimes of high energy physics simply cannot be reached. The energy parameter is the most obvious example. As we have discussed, beam intensities or luminosities are another.

In a sense, accelerator development comes first, and the accelerator parameters define the set of detector problems that must be solved. Needless to say, the accelerator designer must give reasonable attention to the state of the detector and data processing art in limiting his design parameters. This has certainly been the past history of the field, and we believe it will be the pattern of the future as well.

III. PROCEDURE FOLLOWED BY THE SUBPANEL

In carrying out our charge, we have attempted to gather relevant information and opinion from the widest possible sources. The primary focus of our activities has been a series of six two-day meetings. The first meeting, held in September 1979, was largely organizational in purpose. Subsequent meetings were held in November 1979 and in January, February, March and April of 1980. During the November, January and February meetings we heard presentations of the HEP accelerator R&D work at ANL and FNAL, at LBL and SLAC, and at BNL and Cornell. These three meetings were held at FNAL, SLAC and BNL respectively.

All information and opinion-gathering meetings were advertised in advance in the Federal Register. Requests for submission of relevant opinions were published in "Physics Today," February 1980; in the CERN Courier, November 1979; and in a November 1979 mailing to all members of the Division of Particles and Fields of the American Physical Society. Laboratory managements from ANL, BNL, Cornell, LBL, FNAL, and SLAC were invited to address the Subpanel with regard to issues in accelerator R&D.

At our information gathering meetings we heard, in addition to the laboratory presentations, summaries of work abroad and tutorial summaries of specific areas of R&D such as collective accelerators and R&D in superconducting RF devices. Substantial time at each of the meetings was devoted to discussion of general issues in R&D and to the organization and classification of the information we had received.

Acknowledgements

The Subpanel acknowledges with great thanks the very high level of cooperation we have received from all of the laboratories visited and specifically to the large number of individuals at ANL, BNL, Cornell, FNAL, LBL and SLAC who made detailed presentations to the Subpanel. The names and affiliations of these individuals are listed below.

We also recognize with great thanks the hospitality of BNL, FNAL and SLAC in making physical facilities available for our work and to the directors of all of the laboratories participating in our survey for supporting our efforts.

Thanks are also due to the many individuals in Government and University Laboratories and in private industry, both in the U.S. and abroad, who have responded generously to requests for opinion and information relevant to our study.

Presenters

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K. Crook, SLAC
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G. Fischer, SLAC
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IV. STATUS OF HEP ACCELERATOR R&D WORK IN THE U.S.

As a central part of our task, we have assembled an illustrative but not exhaustive catalog of accelerator R&D activities which are directly relevant to the HEP program. The primary purpose of this catalog is to indicate the scope of current work, the magnitude of the overall effort, and the distribution of the effort in terms of short-, mid- and long-range goals. To limit the inevitable ambiguities of definition, we have formulated a working definition of accelerator R&D.

A. What is Accelerator R&D?

The mission of HEP accelerator R&D is to provide the scientific and technological basis for the machines needed to carry out high energy physics experiments. We will adopt the following categories:

1. Research and Development Applied to Projects (RDAP)

   R&D addressed to operating, authorized, proposed, or soon to be proposed projects. We shall also refer to this as short and mid-term R&D.

2. Advanced Accelerator Research and Development (AARD)

   R&D of a more fundamental nature which is anticipated to have benefits for the longer range of HEP programs. We shall also refer to this as long-range accelerator R&D.

Some examples of accelerator R&D we have considered are the following:

1. Discovery and/or reduction to practice of:

   (a) New arrangements of conventional components to produce higher energy, higher currents, or new beams. Examples are: colliding beam linacs and the use of very high energy beams to produce secondary beams for re-acceleration or direct experimental use.

   (b) Methods for producing higher accelerating fields in conventional configurations, for example, superconducting cavities, new cavity design, manipulation of pulsed power, and new RF power sources.

   (c) Methods for producing higher containment fields in conventional configurations, for example, high field superconducting magnets.

   (d) New methods for acceleration, e.g., laser-driven devices, collective methods.

(e) New particle sources at low energies, e.g., guns or ion sources for polarized particles, high current fast modulated guns, high power targets for producing $\bar{p}$ or e^+.

(f) Diagnostic instruments, e.g., polarimeters, emittance measurement devices.

(g) Methods of improving beam brightness, e.g., stochastic and electron cooling and H^- injection.

(h) Beam manipulation devices, such as improved pulsed elements, inflectors, electrostatic or magnetic beam-transport elements, etc.

2. Theoretical, experimental and computer model studies of accelerator phenomena such as high current collective effects, non-linear effects, polarization, etc.

3. Studies whose goal is to optimize design through trade-offs within the boundary conditions pertaining to complex accelerator systems.

4. Development of control systems.

5. Development in allied fields such as materials and vacuum science.

B. Current Activities in U.S. Accelerator R&D

Our listing and description of ongoing work in the field is divided into the thirteen categories shown below. The list is representative and not all-inclusive. This report is not intended as an assessment of the R&D program of any individual laboratory or person in this country. We have, therefore, generally avoided associating the names of particular laboratories or persons with specific endeavors.

1. Experimental accelerator physics
2. Accelerator theory
3. Systems studies of complete accelerators
4. Superconducting magnets and cryogenics
5. Superconducting RF cavities
6. Normal conducting linac structures and cavities
7. High power RF and microwave sources
8. Stochastic (electronic) and electron cooling of beams
9. Particle sources
10. Beam transport
11. Polarized beams
12. Control systems
13. Non-traditional accelerators

1. Experimental Accelerator Physics

Much of the advancement in accelerator science and technology has and will come from studies which use our present complement of accelerators. Most frequently, experiments with these machines are done with the intention of immediate improvement of their capabilities. However, these studies can often produce results which are not only of special interest but can also be widely applied. Recent examples of such work are the detailed exploration of the non-linear resonances produced by focusing structures, the study of the resistive-wall instability and beam-cavity instabilities in linacs and circular accelerators; the characterization of coupling between transverse and longitudinal degrees of freedom, including the head-tail instabilities and the synchrobetatron resonances; and the attempt to measure the various aspects of the beam-beam interaction in colliding beam machines.

Operating accelerators are used for the testing of new devices and accelerator concepts. Examples are the insertion of superconducting magnets and superconducting RF accelerator devices into external beam lines or the accelerators themselves for the study of radiation or other environmental sensitivities and of the possible effects on accelerator beam dynamics. Beam-cooling is also being examined experimentally. The effects of very high frequency fields generated by short, intense bunches are actively under investigation in existing machines.

To assess the general level of activity in the use of existing HEP machines for accelerator physics studies, we sent a questionnaire to accelerator laboratories active in HEP requesting information about the topics studied and about the level of resources devoted to this activity. The responses to these questionnaires are summarized in Appendix IV. Typically, 10% of accelerator running time is used for machine studies. The majority of this time is spent on R&D to improve the present machine performance, and only one quarter on average is devoted to R&D for mid- and long-range improvements. The amount of R&D varies considerably from machine to machine and, as might be expected, tends to be larger for the newer machines.

2. Accelerator Theory

The work being carried out in the U.S. HEP laboratories in accelerator theory can be separated into two categories: accelerator "design" and basic theoretical "research". Accelerator design includes magnetic lattice calcultions, injection and extraction system design, effects of magnet non-linearities, specification of alignment tolerances, RF system studies, and calculations of space charge and other collective limitations. Design also includes systems studies, such as estimates and comparison of colliding linacs versus storage rings and bunched beams versus DC beams in proton storage rings. The distinguishing characteristic of this work is parameter optimization.

The second category has the aim of extending our knowledge of accelerators and the physics of particle beams. Examples are: the theory of microwave instabilities, the theory of long-term stability in non-linear fields, the beam-beam interaction in colliding beam systems, and the study of coherent beam oscillations that are not yet completely understood. This category includes new methods of acceleration, for example, collective field accelerators and laser application to particle acceleration.

In terms of manpower effort, it is important to keep in mind that those working in accelerator theory tend to divide their time between the "design" and "research" categories. We, therefore, will use the units full-time equivalent physicists (FTE).

From information obtained directly from the six large U.S. HEP laboratories, we estimate the total number of physicists working in accelerator theory as 32 FTE. Of these, 11 are in the "research" category, and 21 are in the "design" area. Since the U.S. is supporting a wide range of new projects (the Cornell storage ring which recently turned on, PEP to be operational this spring, ISABELLE under construction, and the Fermilab $\bar{p}$ collider to start construction in the fall of 1980), it is not surprising that almost two-thirds of the effort in accelerator theory is devoted to "design" activity. The fact that short-term goals dominate the theoretical work in accelerators is further confirmed by the paucity of published research in accelerator physics in conference proceedings and refereed journals. This point is illustrated by the following table of the number of papers on HEP accelerator theory "research" whose origins were in the U.S. laboratories in the years indicated.

U.S. National Accelerator Conference (1977) - 4
U.S. National Accelerator Conference (1979) - 5
Nuclear Instruments and Methods (1979) - 0
Particle Accelerators (1979) - 1
Physical Review Letters (1979) - 2

3. Systems Studies of Complete Accelerators

Work in this area ranges from preliminary design of accelerators to long-term work aimed at defining problems relevant to future accelerator needs. The major emphasis is on the former category. Significant efforts are in progress on a study of a linear electron-positron collider in the 100 GeV CM range. Other U.S. system studies include an e^+e^- storage ring for the 100 GeV CM region and an electron ring to be used in conjunction with one (or more) of the existing or future proton machines to study e-p collisions.

Little long-range systems analysis is currently being done.

4. Superconducting Magnets and Cryogenics

By far the largest share of the R&D effort in this field is currently being devoted to supporting the construction of 4 to 5 Tesla magnets for the two major accelerator construction projects at FNAL and BNL. At present, the work focuses on the improvement of magnet fabrication techniques, quality control methods for finished magnets and raw materials, and quench-protection. Systems tests and studies are to provide the knowledge necessary for efficient and safe operation of the complete accelerators. Also, tests and calculations are aimed at elucidating and controlling the techanisms responsible for degraded magnet performance, e.g., "training".

There is, in addition, a small amount of important work aimed at the development of future accelerator magnets with fields up to 10 T. This work has several goals: (1) a detailed understanding of the physics and engineering of superconducting magnet behavior, taking account of the larger forces and stored energy densities in the higher field magnets; (2) evaluation and development of NbTi, Nb_3Sn, and ternary conductors and fabrication techniques for the conductors; (3) development of prototype magnets using geometries other than the Cosine-theta design; and (4) systems studies taking cooling, materials, and metallurgical problems into account.

Accompanying this work on magnets has been a considerable amount of project-oriented development of cryogenic apparatus in general and refrigeration systems in particular. The refrigerators to be used for the accelerator projects now underway will be the largest liquid helium systems ever built and operated. The primary goals in current development efforts are the assurance of operating reliability and the maximization of efficiency.

Attention is being devoted to development of turboexpanders and the removal of oil from the warm helium stream when wet screw compressors are used.

Systems for refrigeration below the lambda point are also in need of development for high field superconducting magnets and RF cavities.

5. Superconducting RF Cavities

RF superconductivity is a technology that holds promise for high accelerating fields with low power input. Excessive power losses and multipactoring at high accelerating fields proved discouraging to the many workers who tried to develop the technology five to ten years ago. The continuing need for economical RF acceleration with high fields, some modest advances in the understanding of previously observed limitations, and improvements in surface science instrumentation have sparked renewed interest in the field. Activities now in progress in the U.S. HEP laboratories include surface studies aimed at the suppression of secondary emission and at understanding mechanisms of excessive local surface losses. Other studies focus on cavity quenching. The principal material being investigated for HEP accelerator uses is niobium. Refrigeration efficiency could be improved if a superconducting material with a higher transition temperature could be found. Nb_3Sn and other materials have been tried in the past but found intractable. At present a small effort to make Nb_3Sn cavity surfaces by a solid-state-diffusion method is underway.

In addition to fundamental studies, cavity design is proceeding with the aim of devising shapes that suppress multi-pactoring, that are simple to fabricate, and that also allow almost complete extraction of power coupled from the beam in unwanted modes.

Considerable effort is also being put into the use of RF superconductivity for intermediate energy (below 1 GeV) accelerators which will aid in the fundamental understanding for HEP applications.

6. Normal Conducting Linac Structures and Cavities

Work on this subject may be divided conceptually into three categories: (1) studies of existing structures to determine their limiting properties; (2) attempts to improve performance of existing designs through relatively minor changes; and (3) development of completely new designs having considerably enhanced properties. In category (1), there is an active program to measure the beam-cavity interaction for very short high-intensity bunches, a subject of great importance for electron colliders. In category (2), there are commercial efforts to increase substantially the gradients of more or less standard structures. In category (3), activity is focused on development of the so-called disk and washer structure within the U.S. nuclear physics program. There is also a small effort to examine structures suitable for use with very powerful sources of optical, millimeter, or submillimeter waves. Finally, there is work on RF quadrupole linac structures which provide both focusing and acceleration at low particle velocities.

7. High Power RF and Microwave Sources

Currently only one of the HEP laboratories is engaged in R&D on RF sources. While the bulk of the effort is in improving the performance of existing tubes, a small amount of work is devoted to development of a klystron in the 100 MW range and of a high efficiency tube of the rotating beam type. The U.S. Nuclear Physics program is supporting a small amount of work on the high-efficiency "gyrocon" concept for application in the UHF band.

Considerable tube development is being carried out by commercial firms under defense contracts or by the Naval Research Laboratory alone or in cooperation with various university groups. After some further developments, some of these devices may have direct application in HEP accelerators. The following is a list of some of the devices which operate in frequency bands now used in accelerators:

Tube Type	Band	Peak Power
Klystron	S	500 MW
Klystron	S	1 GW (built but not tested)
Cyclotron Maser	X	0.9 GW

These tubes have beam voltages of several MV and currents in the kiloampere range.

Another interesting approach is to charge a very high-Q cavity from a CW or long pulse source of moderate power and then to dump the energy from the cavity rapidly by use of a fast-acting switch. The switch in combination with a suitable microwave coupling circuit can effect a large and rapid change in the coupling between the cavity and the external load. Power gains of 5×10^4 have been realized in the kilowatt peak-power range.

8. Stochastic and Electron Cooling of Beams

Part of the ongoing R&D program is aimed at a fundamental understanding of stochastic cooling, and how to improve its utility. Therefore, it includes the theory of stochastic processes, low-noise electronic systems, and advanced beam detectors. Two systems have been built, one to damp transverse oscillations and the other to damp momentum spread. Experiments have begun in a small ring, and momentum cooling has been demonstrated. Another part of the program is aimed at a proposed construction project for which emphasis is placed on momentum cooling. Activities in the program include theory and simulations, as well as pick-up, kicker and electronics development.

Electron cooling R&D has been directed at the development of systems of such a scale as to be useful for realistic accumulation schemes. New diagnostics are being developed to measure the electron beam properties. An advanced electron-collector system allows recovery of 99.9% of the electrons from the cathode and energy recovery of 95%. The conditions necessary to obtain stable operation of the electron system are being studied. This program has entailed interlaboratory collaboration in the design of the gun and collector and in the correction system for the storage ring. The system has been largely completed and is under operational tests. The goals of the experiment are to achieve accumulation of protons by multiple injection and to study high current effects at intensities interesting for a proton-antiproton colliding beam system. There has also been a small activity to extend these studies to the electron cooling of high energy beams.

9. Particle Sources

There is considerable work on ion sources for H^-, H^+, heavier ions and polarized particles within the HEP program and in the nuclear physics and fusion programs. While there are many aspects of sources that require continuing investigation, it is felt that the current level of activity will suffice to meet foreseeable needs.

Recently substantial progess has been registered in electron sources through the development of the laser electron gun. In this device the beam (highly polarized) is generated by photoemission from a cesiated GaAs solid state cathode bombarded by polarized laser light. Peak currents of several tens of amperes have been achieved in short bunches.

10. Beam Transport

Several interesting and important ideas in the transport of bright, high energy electron beams as well as bright, high intensity but low energy electron and ion beams are under intense investigation. The high energy electron work is mainly in conjunction with the linear collider mentioned previously. The problems is to bring a very high energy beam with signficiant energy spread to a very sharp (2 micron) focus without the use of massive magnets close to the focal point. The final focus will be created by means of permanent magnet lenses using a new material (samarium-cobalt) which has the very useful property that its incremental permeability is unity. Broad-band local chromatic corrections are needed and are under design. In addition, one has to transport the high energy beam over a curved path while minimizing the effect of quantum fluctuations in the synchrotron radiation on the phase space density of the beam.

For the focusing of intense low energy beams, an interesting system involving large parallel arrays of miniature electrostatic quadrupoles is under development. It is estimated that very bright, high current beams can be transported and accelerated with minimum space charge effects. For HEP applications such devices could aid in the design of compact injectors and might facilitate the construction of very high power RF and microwave tubes.

11. Polarized Beams

In recent years important advanced have been made in the production of polarized proton beams. Currently, there are two collaborative efforts to apply this work to still higher energy beams. One approach is based on the acceleration of highly polarized protons from a low energy source to high energy in an AG synchrotron. The depolarizing resonances encountered during acceleration are avoided by the use of pulsed quadrupoles. The other approach will provide polarized protons or antiprotons from a secondary beam derived from the decay of polarized lambda hyperons. This beam line will have special magnets to control the direction of the proton or antiproton spins on a pulse to pulse basis.

For electron machines, a number of polarized sources have been developed, one being the photoemission gun mentioned above. Development of this source continues with the goal of improving intensity and polarization. Polarizations in excess of 80% may be achieved.

Studies of the stability of spin motion in electron storage rings with emphasis on providing longitudinally polarized beams are an important aspect of design studies for future possible electron-positron colliding beam devices.

12. Control Systems

The rapid and continuing progress in microelectronics has revolutionized accelerator design. Sophisticated systems for the coordinated manipulation of accelerator parameters, which ten years ago would have been impossible, are now common place. The influence of this revolution is so pervasive that even a partially inclusive catalog is beyond the scope of this report. Suffice it to say that the accelerator community generally has been very adept at capitalizing on each new electronics development. The achievement of reduced operating costs and increased "up time" by sophisticated electronic diagnostics, alarms, and controls have allowed the reduction of operating staffs.

13. Non-Traditional Accelerators

(a) Background

All operating HEP accelerators use magnets and/or accelerator cavities which are direct descendents of the machines of the 30's and 40's. The confinement fields are produced by assemblages of solid conductor and ferromagnetic material. Recent years have seen some serious attempts to break out of this "old" pattern and by so doing to circumvent the limitations of traditional materials and methods. The bolder approaches have focused on the production of very high confinement and acceleration fields directly by manipulation of high-power charged-particle beams in vacuo, or in a plasma medium, or by the manipulation of the charges in a plasma through the action of very intense, very short wavelength radiation passing through the plasma. Other approaches focus on the possible large enhancement of linear accelerator gradients by the use of very high power, very high frequency radiation as from a laser. Yet other activities center on the acceleration and/or confinement of beams having currents much larger than that normally utilized in HEP accelerators. At present, none of these have come to practical fruition in new HEP accelerator machines. Nevertheless, the pursuit of these ideas, and others with similar goals, are an essential part of HEP accelerator R&D.

A substantial portion of this work, in monetary support terms, is conducted outside the HEP program. As a consequence, knowledge of its details is not widely spread in the HEP community. For informational purposes, more details are given in Appendices I, II and III which describe representative ongoing work.

(b) Non-collective-effect accelerators for high-currents

At this time, there is experimental work underway on a number of devices. Among them are an RF linac for high brightness ion beam acceleration, a high-current proton storage ring, several induction linacs for kiloamp beams, and the auto-accelerator in which energy is transferred between a high current charging beam and a relatively small acclerated beam.

Another interesting device, studied only through calculation so far, is the extension of the microwave linear accelerator to optical frequencies. In such a device, the klystron source is to be replaced by a laser and the cavity structure by an optical equivalent such as an assemblage of gratings. It is estimated that effective gradients in excess of 1 GeV/m might be realized with such a device if the very significant practical problems could be overcome.

(c) Collective-effect accelerators

These accelerators can be divided into two broad categories. In one category are accelerators in which the primary purpose of the collective effects is to produce transverse confinement. In the other category, the collective effects produce longitudinal acceleration as well as confinement. Among the former are the magnetically insulated electron-focused ion linac, in which trapped clouds of electrons form a transversely focusing potential well for the ions; and the toroidal electron-focused ion accelerator, which works in a similar way but has a circular equilibrium orbit.

Among the latter are several different types of linear accelerators:

1. The ion-containing electron-cluster accelerator in which an electron bunch containing relatively few ions is

accelerated in some type of linac. In these arrangements, the energy gain of the ions will be roughly (ion mass)/(electron mass) x electron energy gain. Past work on these devices, such as the Electron Ring Accelerators, revealed great difficulties in their realization, and very little activity continues in the U.S.

2. Moving-potential-well accelerators exist in several manifestations, most of which involve high current electron beams being shot through foils into plasma volumes with various auxiliary focusing devices. These accelerators are characterized by rather low ion energies with large currents and appear to have little interest for HEP applications directly. They are described more fully in Appendix I.

3. Two devices are being developed which use traveling waves "grown" on an intense relativistic electron beam to achieve acceleration and focusing. In one, cyclotron waves with tapered phase velocity are grown on the beam. In the other, the more familiar space-charge waves are grown on the beam as it passes through an exciting slow-wave structure. Conceivably, such accelerators might find some direct use in HEP machines or be part of very high-power RF, microwave or optical amplifiers whose radiation output is used to drive an accelerator.

4. Another interesting class of accelerators has been proposed in which the very high fields at the focus of a laser beam (or other beam of millimeter or sub-millimeter wavelength) gather up a high-density pulse of electrons from a low energy electron stream or dilute plasma and propel it forward with a controllable group velocity. Ions can then be acceleratd in the wake of this moving charge pulse. It is thought that gradients of 30 GeV/m might be achieved. So far only analytical estimates and some computer simulations have been carried out. In Appendix II, several of the suggested "laser" accelerators are reviewed briefly and a representative bibliography is given.

(d) Power Sources

The practicality of several of the very high gradient linear acceleration devices mentioned above depends in part on the existence of very high peak power sources in the millimeter to optical bands. A wide range of work is being carried out within the fusion and defense programs, and is reviewed briefly in Appendix III.

C. Resources Devoted to R&D

The resources devoted to work that is clearly of a long-range nature are quite small, certainly not more than $3M per year.

By far the greatest portion of monetary and manpower resources in R&D is applied to the support of operations and approved construction projects, with a much smaller amount applied to anticipated construction proposals. We would classify this as short- to mid-term R&D (RDAP). Figure 4 is a graph of R&D support as a fraction of the total HEP operating budget, which is about $200M in this fiscal year (FY 80). Currently, the total expenditure for accelerator R&D is about 14% of operating funds, 3/5 of which is devoted to superconducting magnet R&D in support of the two current major construction projects.

D. Manpower Resources in Accelerator R&D

We have attempted to study the utilization and the supply of Ph.D. level professional scientists working in the accelerator field. For the study of the characteristics of the field, we have used the data presented in the 1978 census of the U.S. High Energy Physicists and Graduate Students (DOE/ER-0019, November 1978). For questions relating to the future supply of accelerator scientists, we chose to concentrate on the group which entered the field during the last five years. The data presented were compiled from information supplied by the five U.S. National Laboratories - Argonne, Brookhaven, Lawrence Berkeley, Stanford Linear Accelerator and Femilab - which have had substantial high energy physics related accelerator work in progress during the last five years.

Figure 5 shows the age distribution of accelerator physicists. As can be seen, there is little difference between the age spectra of those who reported accelerator physics as their primary activity or as their secondary activity. The mean age is around 49 years and the distribution is broad. Figure 6 shows the age distribution by subfield. Of some concern is the rather old age distribution of accelerator theorists as of 1978. The figure also illustrates that the majority of accelerator physicists (84%) are engaged in design and development while relatively small segments are primarily involved in theory (9%) and operations (7%).

In general, the average age of the accelerator physicist is about 49 years and the distribution has a broad peak at age 40 years with very few in the 25 to 35 year range. We have found that this distribution reflects the fact that many accelerator physicists have entered the field substantially after receiving their Ph.D. in other areas. Figure 7 shows the distribution in age at the time of entry into the accelerator field for entrants during the last five years.

Figure 7 shows a nearly flat distribtion between ages 25 and 45 indicating a pattern of delayed entry into accelerator physics. In the comments made by the Laboratory managements, in response to our requests for manpower

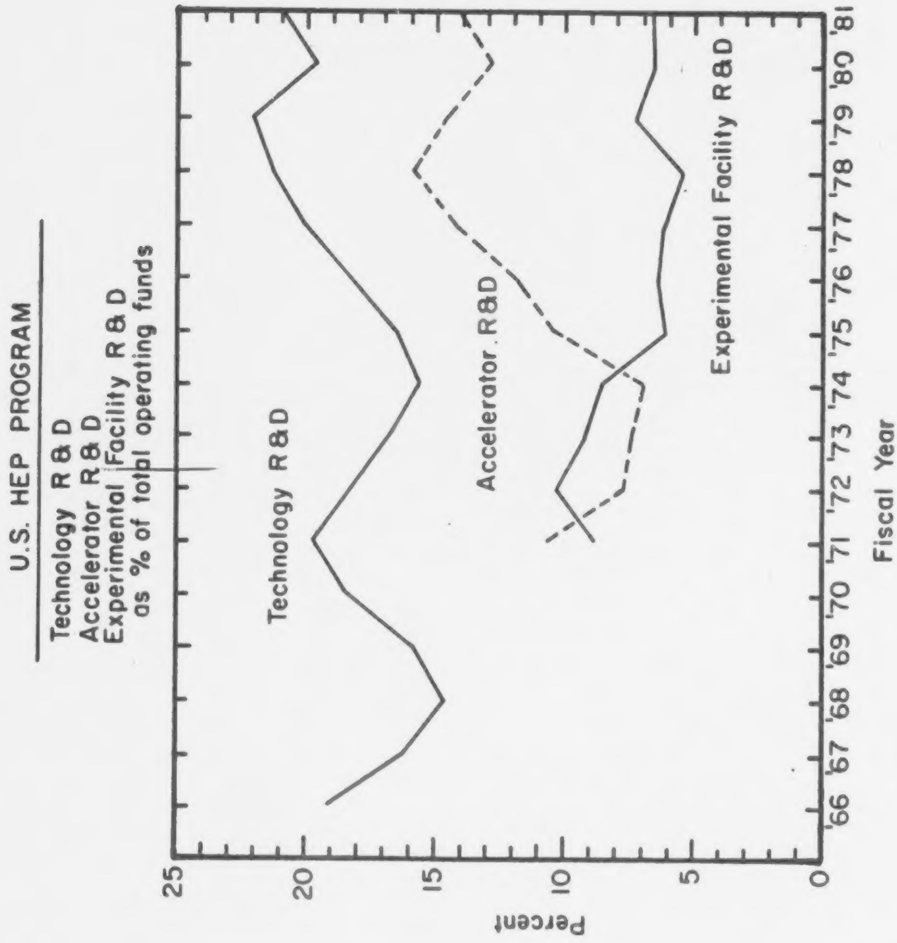

U.S. HEP PROGRAM
Technology R & D
Accelerator R & D
Experimental Facility R & D
as % of total operating funds
Technology R & D
Accelerator R&D
Experimental Facility R & D
Percent
25
20
15
10
5
0
'66
'67
'68
'69
'70
'71
'72
'73
'74
'75
'76
'77
'78
'79
'80
'81
Fiscal Year

Fig. 4

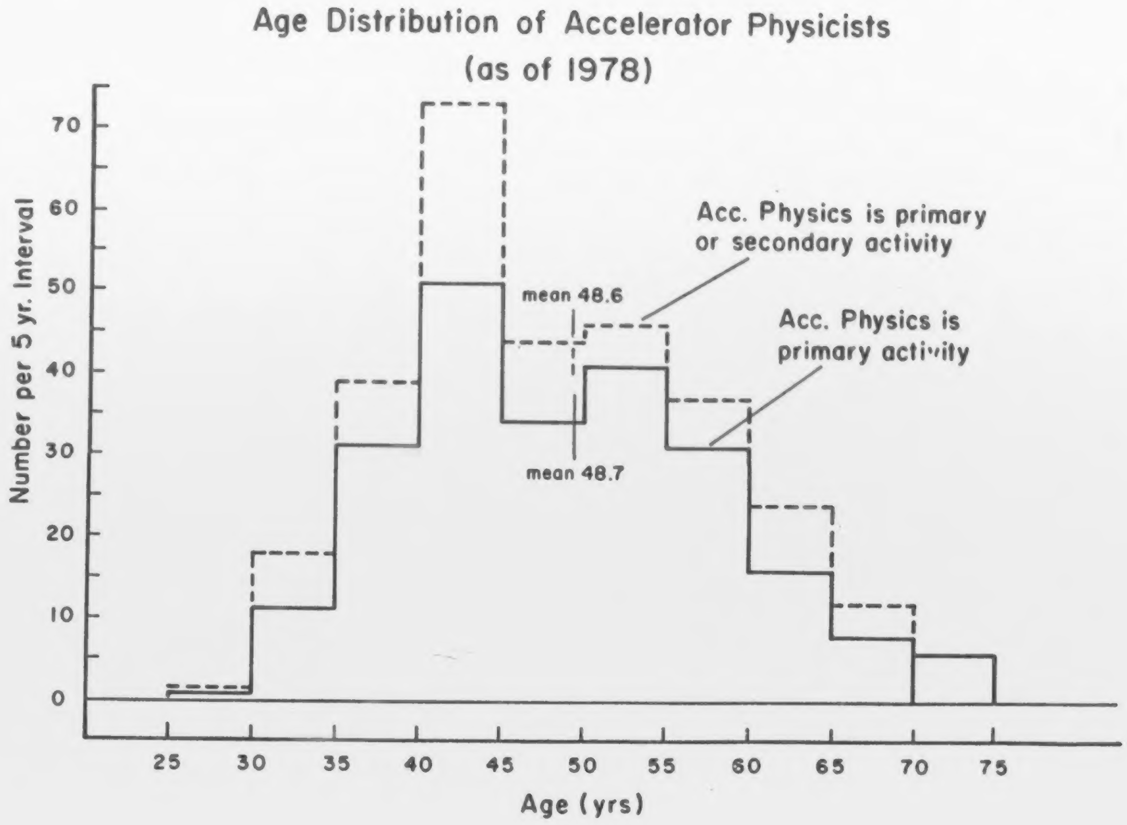

Age Distribution of Accelerator Physicists
(as of 1978)
Number per 5 yr. Interval
0
10
20
30
40
50
60
70
25
30
35
40
45
50
55
60
65
70
75
Age (yrs)
Acc. Physics is primary or secondary activity
Acc. Physics is primary activity
mean 48.6
mean 48.7

Fig. 5

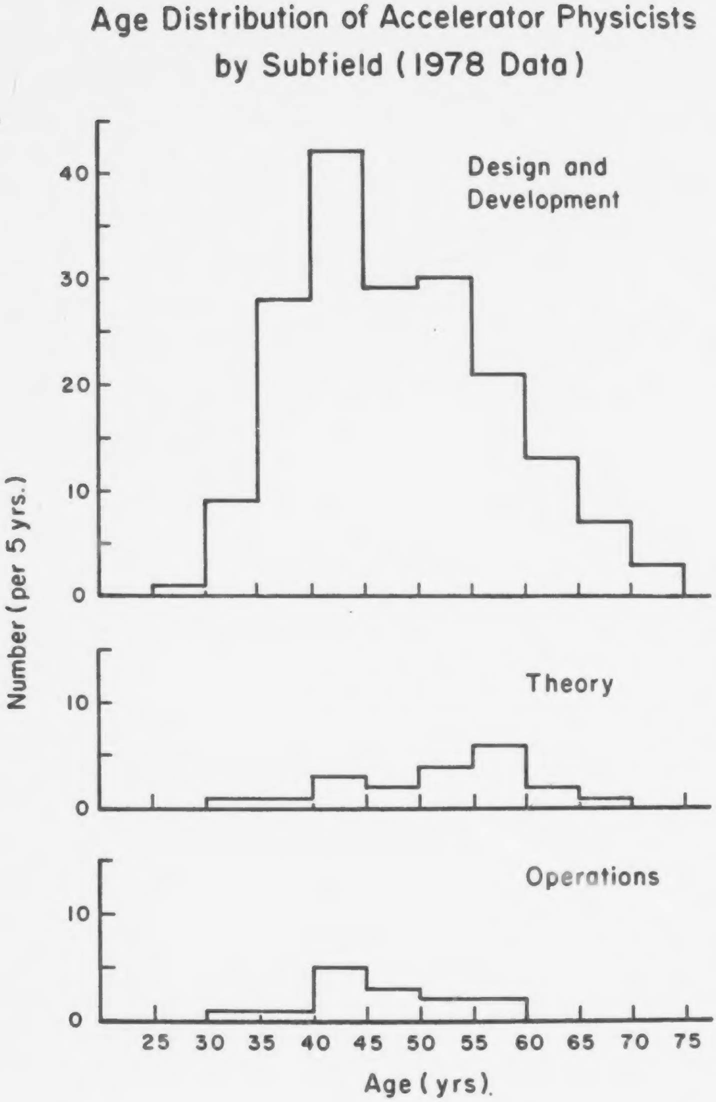

Age Distribution of Accelerator Physicists by Subfield (1978 Data)
Design and Development
Theory
Operations
Number (per 5 yrs.)
0
10
20
30
40
0
10
0
10
25
30
35
40
45
50
55
60
65
70
75
Age (yrs)

Fig. 6

Distribution of Age at Time of Entry to Accelerator Physics (Last 5 years)

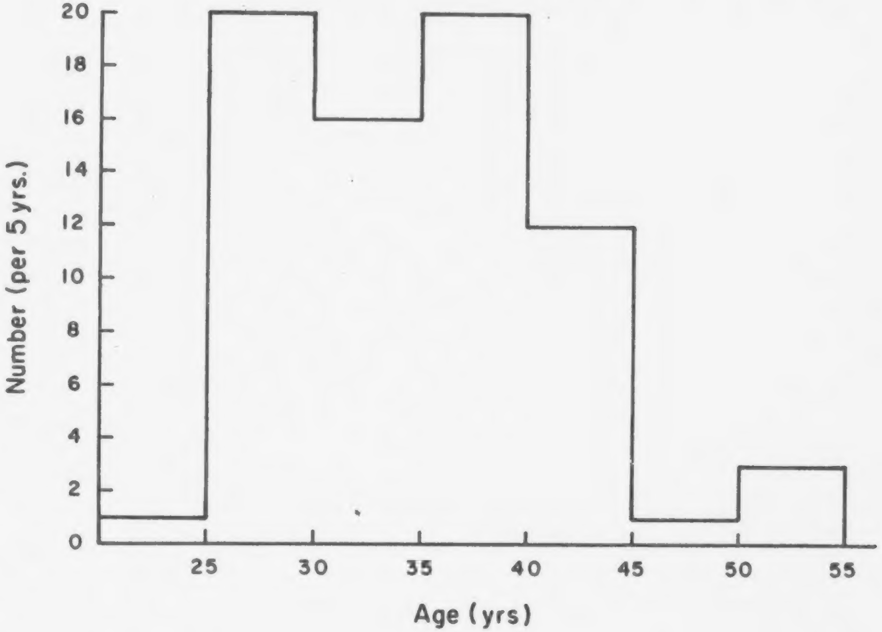


Fig. 7

information, there was a common theme of stressing the importance to the accelerator field of these post Ph.D. entrants.

We have also determined the field of the Ph.D. dissertation for the entrants of the last five years as shown in Table 4 below.

Table 4

Distribution by Field of Ph.D. Thesis for Entrance to Accelerator Field in the Last Five Years

Ph.D. Thesis Field	ANL	BNL	Cornell	LBL	FNAL	SLAC	Total
Accelerator Physics		1	3	2		1	7
Experimental High Energy Physics	2	2	2	2	26	3	46
Theoretical High Energy Physics	1	3		1	1	1	7
Plasma Physics	1		1	3			5
Other		4		3	2	1	10
						TOTAL	75

Table 4 shows that although there are diverse sources of accelerator physicists, a very important, even dominant, component is high energy physics. It should be noted that there are a number of physicists who make contributions to both particle and accelerator physics. In fact, it is this overlapping involvement which gives a great deal of strength to high energy physics.

Although the numbers of working accelerator physicists trained initially in the field is small, it is in a qualitative sense very important. These people tend to be broadly trained and will play a role greater than their numbers indicate in the areas of long-range accelerator research and development. In addition, the existence of faculty and dissertation programs in accelerator physics is very important for the collection and communication of results obtained in the field. Many of the post-Ph.D. entrants to the field gain their knowledge of the subject from key books and articles as well as by their "on-the-job" training in their new profession.

We have also compiled data on the Ph.D. institutions of the entrants of the last five years. These are presented in Table 5. At the multifunction laboratories, a fraction of the physicists enumerated in Table 5 work in non-HEP accelerator areas. This fraction is small and its exclusion would not change the overall pattern.

Table 5

Ph.D. Granting Institutions for Entries into Accelerator Field in Last Five Years

Institution	BNL	FNAL	LBL	SLAC	ANL	Cornell	Total
U. of British Columbia	-	1	-	-	-	-	1
U. of Calif.-Berkeley	1	4	1	-	2	-	8
U. of Calif.-Irvine	-	-	1	-	-	-	1
U. of Calif.-Los Angeles	-	-	1	-	-	-	1
Cal. Tech.	-	1	-	-	-	1	2
Case Inst. of Technology	-	1	-	-	-	-	1
U. of Chicago	-	3	-	-	-	-	3
Columbia	-	2	-	1	-	-	3
Cornell	2	-	-	-	-	4	6
Harvard	-	1	1	-	-	-	2
U. of Illinois	-	3	-	-	-	-	3
Iowa State U.	-	1	-	-	-	-	1
Johns Hopkins	-	1	-	-	-	-	1
U. of Maryland	-	-	3	-	-	-	3
Mass. Inst. of Tech.	2	-	1	2	-	1	6
U. of Michigan	1	1	-	-	-	-	2
Minnesota	1	-	-	-	-	-	1
North Texas U.	-	1	-	-	-	-	1
U. of Pennsylvania	1	-	-	-	1	-	2
Pittsburgh	1	-	-	-	-	-	1
Princeton	1	-	-	-	-	-	1
Purdue	-	1	-	-	1	-	2
U. of Rochester	-	1	-	-	-	-	1
Rutgers	1	-	-	-	-	-	1
Stevens Inst. of Tech.	-	-	-	-	-	-	-
Stanford	-	-	-	1	-	-	1
SUNY, Stonybrook	1	-	-	1	-	-	2
Tufts	1	-	-	-	-	-	1
U. of Washington	1	1	-	-	-	-	2
U. of Wisconsin	1	3	-	-	-	-	4
Yale	2	-	-	-	-	-	2
Foreign	2	3	3	1	-	-	9
Totals	19	29	11	6	4	6	75

In summary, we find that the supply of accelerator scientists while perhaps marginal, is not in a critically low state. A key component in the supply is the migration into the field by physicists trained in other areas, the most important being experimental high energy physics. It is clear to us that this will continue to be the case for some time. Activities such as graduate courses, lecture series at the laboratories, etc., which educate high energy physicists in aspects of accelerator physics and which convey some of the intellectual content and challenge of the field are important to the continued, necessary recruitment of accelerator scientist and should be encouraged. While the number of accelerator physicists originally trained in the field is small, they and the programs which produce them are important and should be encouraged and supported.

E. Resource Allocation for Accelerator R&D

A very small amount of R&D is carried out by university groups operating independently of the national accelerator labs. Proposals are made directly to the agencies and subjected to the same peer review process used for HEP particle physics research activities. Some work is carried out by university groups in collaboration with the large accelerator labs. In these cases, the initiative may be from the university or shared with the labs; funding may be through the laboratories or by some arrangement with the agencies.

At the national laboratories, the patterns are diverse. Initiatives come from directors, laboratory accelerator staffs, in-house HEP physicists and sometimes from "user" physicists. These may be reviewed by the laboratory management, alone or with the advice of various program advisory or visiting committees. In consequence, internal laboratory funds are allocated, or formal proposals may be forwarded to the agencies for review and support.

The flexibility for reallocation of internal laboratory funds is closely related to the total amount of HEP funding at a given laboratory. At the multipurpose laboratories this flexibility may be limited by the ongoing needs of other programs for accelerator expertise and relative priorities must be set by the laboratory management. On the other hand, the management can draw upon expertise, normally supported by other programs at these laboratories, both in the accelerator field and in other disciplines.

Accelerator R&D is a part of the "technology" line item in DOE (but not NSF) budgets, but the accounting and reporting practices with regard to it vary among the labs, which makes meaningful comparisons difficult. Nevertheless, it is clear that the larger HEP operating budgets of the HEP accelerator laboratories allow considerable flexibility in the continual tuning of the balance among various R&D activities and in the rapid start-up of termination of small R&D activities.

The above remarks do not apply to R&D in support of approved construction projects. These funds are more formally programmed and managed as an integral part of the construction projects.

V. ACCELERATOR R&D EFFORTS ABROAD

Accelerator R&D outside the U.S. is concentrated in three well-defined international regions: Western Europe, the USSR and Japan. These geographic subdivisions comprise those advanced nations whre the combination of cultural impetus and economic resources naturally lead to the pursuit of an advanced research enterprise such as high energy physics. One also finds in these regions the broad base of advanced technology which forms the essential foundation for the successful pursuit of such research.

A. HEP Accelerator R&D in Europe

Accelerator R&D in Europe is concentrated largely at CERN and DESY, with some accelerator-related R&D taking place in a few other laboratories.

1. Accelerator Physics

Accelerator physics studies are carried out on the PS, ISR and SPS at CERN, and on PETRA and other accelerators at DESY. Approximately 20% of the running tme of the ISR is devoted to machine development; the corresponding figures at the SPS and the PS are 15% and 10%, respectively. For PETRA, which is still in development, the figure is 30%. Much of this work is directed toward improving machine performance, but a significant fraction has longer term aims and implications. Examples are the original development of stochastic cooling on the ISR, studies of "microwave" instabilities on the PS and the SPS, lifetime of bunched beams on the SPS, and the beam-beam interaction on the ISR. The luminosity of PETRA is smaller than planned by about an order or magnitude, and much machine study is devoted to understanding the limiting effects. This has the short-term aim of improving the luminosity, but the understanding of this problem may have long-term implications for LEP or LEP-like projects. Studies are also devoted to the upgrading of PETRA to 22 GeV per beam.

CERN has built a special beam cooling experiment, ICE, on which experimentation with stochastic and electron cooling was performed.

2. Advanced Technology

(a) Superconducting Magnets

Studies of superconducting accelerator magnets started long ago at the Rutherford Laboratory, Saclay, and Karlsruhe. In 1970, these laboratories joined with CERN in a collaboration, GESSS, for coordinating their activities toward equipping the SPS with superconducting magnets. Since then, the original purpose has fallen to the background, and these laboratories have turned to other applications of superconductivity, e.g., fusion.

CERN has continued work on superconducting magnets for beam transport in the experimental areas of the SPS (in collaboration with Saclay) and for a low-beta insertion in the ISR.

Of longer term interest is the work in France (Saclay and Grenoble) on high field magnets based on NbTi, and the Nb_3Sn work at the Rutherford Laboratory.

(b) RF Superconductivity

Karlsruhe has been active in the field of RF superconductivity for many years. They built a small proton linac with a helix structure and encountered problems (multipactoring, etc.) that limited the accelerating fields to modest values. In collaboration with CERN, they built a superconducting RF separator, which has worked very reliably for several years.

A collaboration involving CERN, DESY and Karlsruhe is making model superconducting cavities to be tested on DORIS. RF superconductivity work has also gone at Wuppertal (Germany), Genoa (Italy) and Orsay (France), on a smaller scale. Currently, all these laboratories are pooling their efforts with CERN to make experimental cavities aimed specifically at LEP application.

(c) Vacuum Development

R&D on vacuum technology has high priority at CERN in the continuing effort to improve the performance of the ISR.

3. Project-oriented R&D

Currently, there is one accelerator project under construction in Europe and several others are under study. All of these required extensive R&D of various kinds. We list main features below.

(a) $p\bar{p}$ at CERN

The main aim is to achieve 2 x 270 GeV $p\bar{p}$ collisions in two experimental areas of the SPS, with a luminosity of 10^{30} $cm^{-2}s^{-1}$ in each. This project relies on stochastic cooling for accumulating enough antiprotons, and extensive theoretical and experimental ICE studies are near completion. Studies, both theoretical and experimental, have been undertaken on lifetime limitations, the beam-beam interaction and other instabilities.

The ISR is also being prepared to receive antiprotons. In addition, there is a program for low energy antiproton beams.

(b) LEP

The study of LEP is centered at CERN but is carried out in collaboration with ECFA and most of the HEP national laboratories in Europe. LEP's main parameters are:

Energy	2 x (50-130) GeV
Luminosity	$0.4 - 1 \times 10^{32} cm^{-2} s^{-1}$
Circumference	30.6 km

Many people work full-time on the study of this project. It is difficult to distinguish between R&D and design based on advanced technology, but a considerable fraction of the effort should be classed as R&D. Examples are beam studies and optimization of parameters to steer clear of instabilities up to design luminosity. The RF superconductivity studies have been mentioned earlier, and the highest energy quoted above assumes superconducting cavities. "Pulsed" conventional cavities are also being studied.

(c) HERA

The study of this project is centered at DESY but assistance is drawn in from ECFA, CERN and other laboratories. It is a colliding-beam facility with an electron ring interlaced with a superconducting proton ring:

Energy	30 GeV e - 820 GeV p
Luminosity	$10^{31} - 10^{32} cm^{-2} s^{-1}$
e longitudinally polarized	
Circumference	6.5 km

The project study leans heavily on the experence being gained at PETRA. Superconducting magnet R&D is being carried out in collaboration with other institutions.

B. Accelerator R&D in Japan

The accelerator R&D work in Japan is managed and funded as a small part of the effort of laboratories operating large accelerator facilities. Because experimental high energy physics is a relatively new endeavor in Japan, the large laboratories are fewer in number and not as large as those in the U.S. Compared to the U.S., they do receive much more assistance and support from industry, where a great deal of capability and depth for doing accelerator R&D work resides. Japanese high energy laboratories operate with a relatively small accelerator R&D staff and contract most of the R&D to industry. The only two high energy laboratories in Japan are the National Laboratory for High Energy Physics in Tsukuba and the Institute for Nuclear Study of the University of Tokyo.

1. National Laboratory for High Energy Physics (KEK)

This laboratory built and is now operating a 12 GeV proton synchrotron. The accelerator R&D program is mostly related to the proposed high energy colliding beam facility TRISTAN (Transposal Ring Intersecting Storage Accelerator in Nippon). This facility would consist of two intersecting rings having an average radius of 432 m. The electron ring will be used for 2 x 30 GeV e^+e^- colliding beams and will be composed of conventional magnets and superconducting RF cavities.

Four accelerator physicists are engaged in the development of superconducting RF accelerating structures. They have studied C-band Nb cavities and obtained a maximum axial field of approximately 19 MV/m. They also have succeeded in accelerating a continuous electron beam from 0.08 MeV to 0.64 MeV in a 9-cell structure, and are designing 500 MHz accelerating cavities for the TRISTAN electron ring.

A group of about ten people is working on the development of superconducting magnets. A model dipole for TRISTAN is in construction. The design and the conductor used are similar to those of the FNAL energy-saver dipole. There are other superconducting magnets in various stages of construction.

In collaboration with the Japanese National Research Institute for Metals (NRIM), KEK plans to construct a 7-8 T dipole using V_3Ga multifilament superconductor developed at NRIM. They will also soon try out a 7 cm aperture 35 cm long dipole using Nb_3Sn conductors.

2. Institute for Nuclear Study (INS), University of Tokyo

The major accelerator project proposed for the INS is the Numatron, a heavy ion synchrotron capable of providing heavy ions up to uranium in an energy range of 0.1-1.3 GeV per nucleon. The R&D effort related to this construction project is concentrated on a Test Accumulation Ring for Numatron (TARN). This is a 5.06 m radius ring for the study and development of the injection stacking schemes and the high vacuum system. The construction of TARN was completed last year. An average pressure of 2×10^{-11} Torr was attained after 50 hours of baking at 250°C and 1000 hours of pumping. Injection studies started in August 1979. Simultaneous betatron and momentum stacking was successfully performed, but it has not yet been possible to make a reliable measurement of the efficiency.

The University of Tokyo group is building a 5 T, 13 cm aperture, 6 m long superconducting solenoid for the KEK muon channel.

3. Superconducting Work in Industry

Several companies are engaged in the development of high field superconducting wires. Furukawa Company is developing Nb_3Sn and V_3Ga wires, and Sumitomo Metal Company is developing Nb_3Sn wire.

C. HEP Accelerator R&D in the USSR

Accelerator R&D in the Soviet Union is carried out at a number of different institutions and is broad in scope. Our information about the Soviet program may be incomplete. Some of the Soviet work is reported at international conferences and published in Western journals or translations of Soviet journals.

1. Accelerator Physics

We have no information on the relative amount of accelerator running time or operations budgets devoted to accelerator physics. There is much theoretical activity concerned with all aspects of accelerator physics, most notably in stochastic and electron cooling, non-linear beam dynamics, collective phenomena and polarized beams. Most of this activity is centered at Serpukhov and Novosibirsk.

2. Advanced Technology

(a) Superconducting magnets

Much of the activity is related to the UNK project discussed below. This work is being done at Serpukhov, Leningrad, Moscow and in collaboration with Saclay. The basic design involves the use of cold bore magnets with warm iron.

(b) High power sources

Important developments in high power RF sources have been made at Novosibirsk with the gyrocon development.

(c) Ion sources, heavy ion accelerators, collective accelerators

There is interest in these topics at several institutes, in particular, Novosibirsk, Dubna, and Moscow.

3. Project-oriented R&D

There are two major projects under serious consideration in the USSR, UNK and a high energy e^+e^- linac collider.

(a) UNK

The main project which appears to be moving ahead is at Serpukhov, 5 km north of the present facilities. This is a proton-based facility employing the existing 70 GeV synchrotron as an injector. A large tunnel (4.5 m section) of circumference 19.2 km will house, ultimately, one conventional accelerator ring, to reach 400-700 GeV, and two superconducting accelerator rings to reach 3 TeV for fixed-target physics and 6 TeV for colliding beams physics (pp). There is discussion of doing ep colliding beams and also discussion with Novosibirsk to perform $p\bar{p}$ in the conventional ring. The R&D for UNK is centered at Serpukhov. Existing groups are carrying out R&D programs, sometimes in collaboration with other institutions. For example, the superconducting magnet program is being carried out with Saclay, Leningrad, and others. The magnets will be built at Serpukhov.

(b) Linac colliders

The Novosibirsk institute is involved in a design study of linac colliders for e^+e^- in the multihundred GeV range. There does not yet seem to be a well-defined project, however, the institute is carrying out R&D in related areas, such as RF power sources, field breakdown tests in cavities, cavity design, etc.

VI. RELATIONSHIP OF U.S. ACCELERATOR R&D TO EFFORTS ABROAD

Accelerator R&D, like research in high energy physics which it supports, has a strong international flavor. The relationship of the U.S. accelerator R&D effort to the other international programs which have been described is one of mutual support. The good communication that exists between U.S. physicists and those of the regions, particularly in western Europe, has permitted a sharing of a common pool of knowledge and techniques.

The U.S. accelerator R&D effort has been highly successful in the application of advanced concepts to high energy physics. The accelerator R&D efforts within other regions, particularly western Europe and the USSR, have been of substantial benefit to the U.S. This can be seen by some recent imports: a stochastic beam cooling, invented and first developed in Europe; the initial demonstration in the USSR of beam cooling via a cold electron stream, and the development in the USSR of negative ion sources used in the U.S. for charge-exchange injection. In a technology-dominated enterprise such as accelerator R&D, the overall advances require the contributions of the widest possible spectrum of talents and specialists. It is in this sense that the interchange between U.S. and foreign experts benefits both, and it is for this reason that maintenance and further cultivation of the mechanisms for exchange of ideas and information among the various regions active in HEP will yield further dividends.

A detailed, quantitative comparison between U.S. accelerator R&D and that in the other three international regions is difficult because of differences in definition as to what constitutes accelerator R&D, differences in accounting methods, and variations in the means of financing. Any such comparison should take account of the current U.S. allocation of approximately 3/5 of present accelerator R&D funding to research on superconducting magnet technology for the Energy Saver and ISABELLE.

The Subpanel has examined the relation of U.S. accelerator R&D to that in the other regions on a qualitative basis by looking at manpower, current projects, and styles of doing R&D. Accelerator R&D in Japan is a modest effort compared to that in the U.S., western Europe and the USSR. Western Europe has, in comparison to the U.S., a somewhat larger theoretical and experimental effort. The number of Ph.D. physicists available for this work is larger than in the U.S. and they are well supported.

Accelerator R&D in the USSR is recognized to be large and well-funded. Any examination of USSR accelerator R&D must, of course, take special note of the creativity at the Institute of Nuclear Physics at Novosibirsk. This institute, under the leadership of the late G. Budker, has become a world-recognized and admired source of original and ingenious ideas in accelerator technology.

In summary, the Subpanel finds that vigorous and productive accelerator R&D efforts exist abroad, that the efforts in western Europe and the USSR are larger in aggregate than in the U.S., that these efforts are complementary in part to the U.S. approach, and that the strong international flavor is of general benefit to the field.

VII. CRITICAL REVIEW OF U.S. ACCELERATOR R&D EFFORTS

A. Challenges and Prospects

As previously observed, the history of accelerator development is a remarkable record of the continuing introduction of new technologies and inventions. These innovations have yielded an exponential rise in accelerator energies through sharp reduction in unit energy costs of accelerators. The technologies we are now just bringing into practice should permit the continuation of this trend through the next generation of accelerators. As we look beyond this next generation, however, to the accelerators we will begin to need roughly ten years from now, it is clear that current technologies must be developed further and that entirely new inventions and technologies will be necessary.

The role of innovation was shown clearly in the Livingston chart of Figure 1. One saw there that each type of accelerator has gone through an evolutionary cycle and was then replaced by a more advanced type of instrument. This succession can also be shown graphically in unit cost terms. In Figure 8, the cost per (center-of-mass) energy unit for various proton accelerators is displayed versus the year of first operation of that accelerator. Such costs can only be approximate since each accelerator installation was made under special circumstances which affect its cost. Moreover, individual accelerators differ substantially in terms of ancillary facilities. Cost comparisons between U.S. and foreign installations are difficult to interpret quantitatively due to the vagaries of exchange rates, combined with variations of true purchasing power of foreign currency. For all these reasons, the costs indicated in Figure 8 should be viewed only as qualitative indicators.

The dramatic decrease in unit costs has been brought about by many factors, the most important being the introduction of new inventions and technologies by accelerator builders themselves. Crucial to these innovations has also been the general advance of modern technology in such areas as electronics, materials and manufacturing technique, vacuum, cryogenics and the like. To make the role of new accelerator technologies clear, each point in Figure 8, which corresponds to a particular accelerator, is plotted with one or more labels which indicate an important innovation used in that accelerator. While these are by no means the only important ideas used in each of these machines, we have chosen to simplify by restricting the list to the weak-focusing principle, the strong-focusing principle, superconducting magnets, the colliding-beam technique, and beam cooling. Also of note are the ingenious uses of existing accelerators as injectors for the new accelerators or of adaptation of existing facilities to produce significantly higher energies. All of the above have been central to maintaining the downward cost trend. The "saturation" of a given technology is seen in the upper part of Figure 8. The unit cost for fixed technology remains about the same. The downward trend in unit costs shown by the graph is associated with the introduction of new technologies.

INCREMENTAL COST PER BEAM ENERGY UNIT vs YEAR
M(1979)$/GeV-CM
PROTON MACHINES

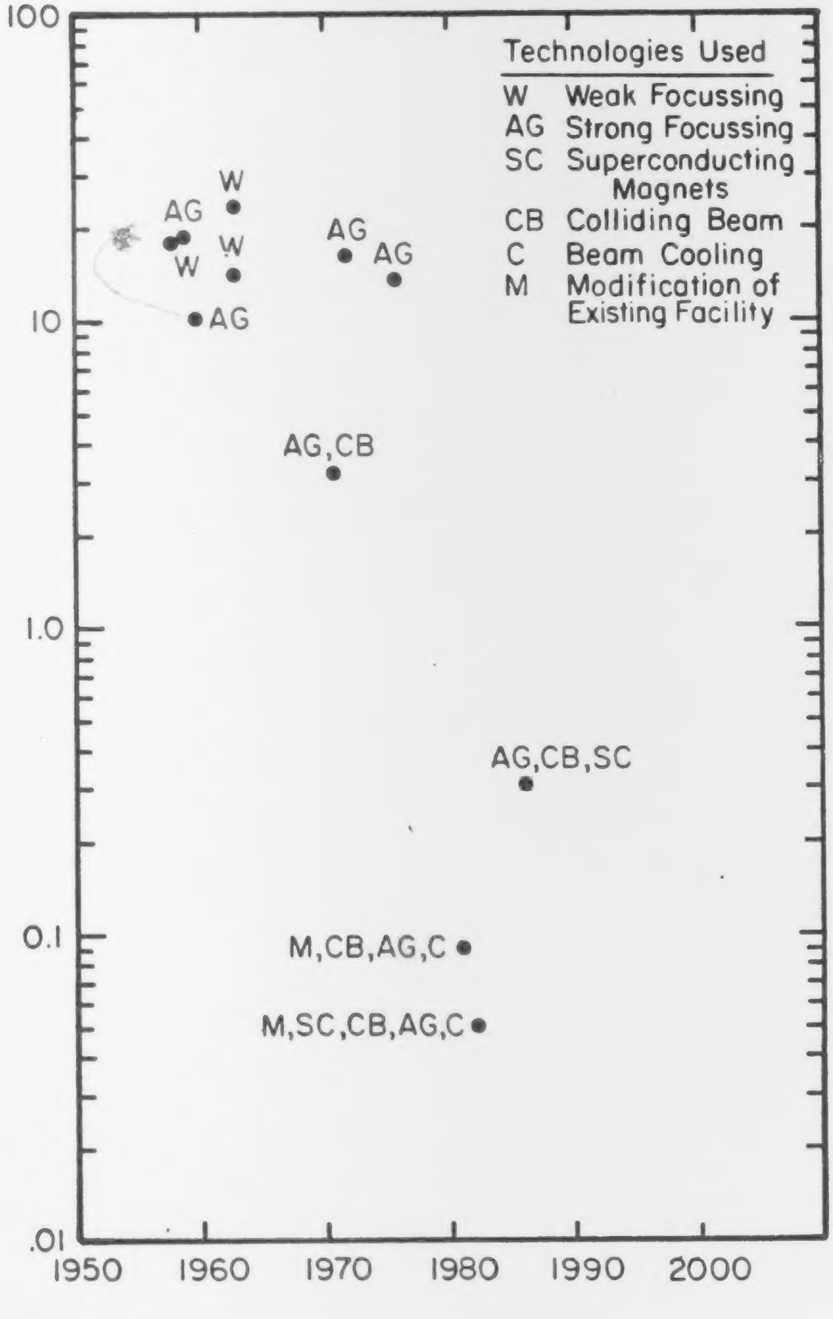


Fig. 8

One may combine the information of Figure 8 with the Livingston plot to make projections about what one may expect for future facility costs under various circumstances. We have made a crude "fit" to the points in Figure 8. From this curve, one can obtain the unit cost of an idealized "Livingston Accelerator" for any given time. This is then multiplied by the Livingston plot CM energy at that time to yield an idealized cost for a facility of that energy. The result is shown together with the Livingston line in Figure 9. The total cost (in 1979$) for the idealized machines, prior to 1980, is shown by the top curve. For purposes of illustration, thr~~ee different co~~st curves are shown for the period after 1980. The middle curve is a continuation of the pre-1980 line. The top curve shows what would happen if innovation were to cease. The bottom curve shows an ideal case in which our R&D is so successful that costs decline at the same rate that energies rise. If one makes the reasonable assumption of constant purchasing power available to the field, then the circumstances described by the top, "no new technology," cost curve are clearly unacceptable. In this situation, we would be unable in the future to build any new accelerators, and our elementary particle physics would soon wither away. Even if we maintain our historical, very high, rate of progress, the middle cost line shows that, with constant purchasing power available, the number of facilities must continue to shrink if we are to provide individual facilities that, in time average, keep us on the "Livingston Line." If we are able to innovate at a rate sufficient to maintain the lower cost line, then we can meet the energy goal indicated by the "Livingston Line," while maintaining the current level of diversity of facilities.

We have tried to compile an equivalent picture for electron machines, as shown in Figures 10 and 11. Owing to possible ambiguities in the interpretation of CM energy for fixed-target electron machines, we have made our plot of unit cost, Figure 10, in terms of beam energies. All the new HEP electron machines after 1967 are colliding beam machines so that unit CM energy costs can be obtained by halving the plotted numbers for these newer machines. The electron picture is made complex by the fact that under conditions of constant technology, unit costs rise as the square of the CM energy due to synchrotron radiation. This is shown in the top cost curve of Figure 11, which rises at twice the slope of the "Livingston Line." The "current trend" line is based on two points only. At the 1980 point we have used established, current costs. At the 1989 point, we have used the LEP estimates, translated into dollars by the factor 0.5 x SF = $. This factor, of course, cannot be very precise because of well-known uncertainties in accounting methods and currency-exchange rates. Even if the factor given is off by 50 percent, Figure 11 shows that the need for innovation in electron machines is equally, if not more, urgent than for proton machines.

While we have dwelt on unit capital costs for providing particle energy, one must not lose sight of the needs for maintaining or improving luminosities, providing extracted beams, polarized beams, etc., while

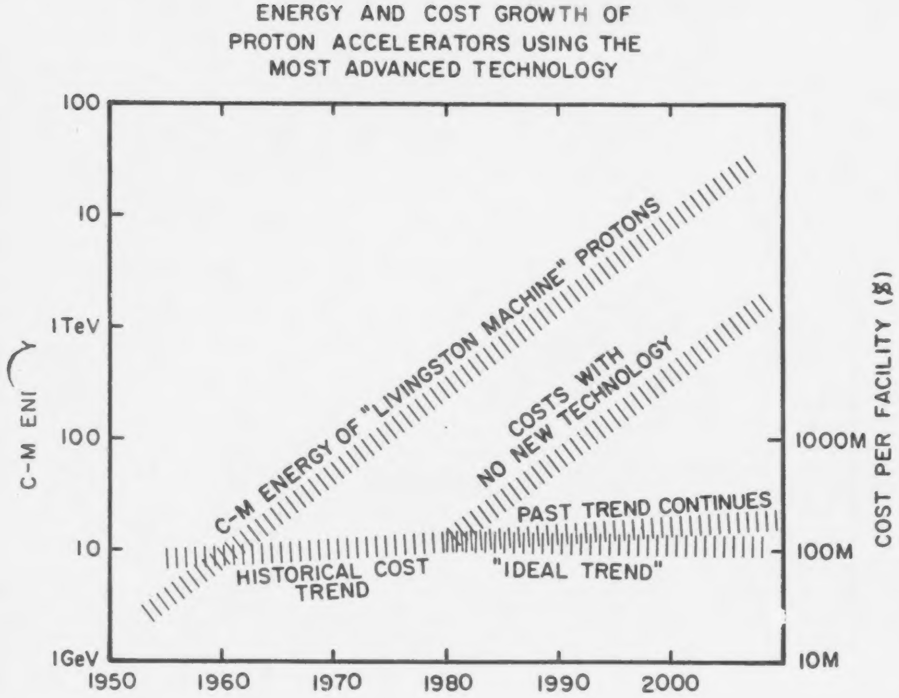
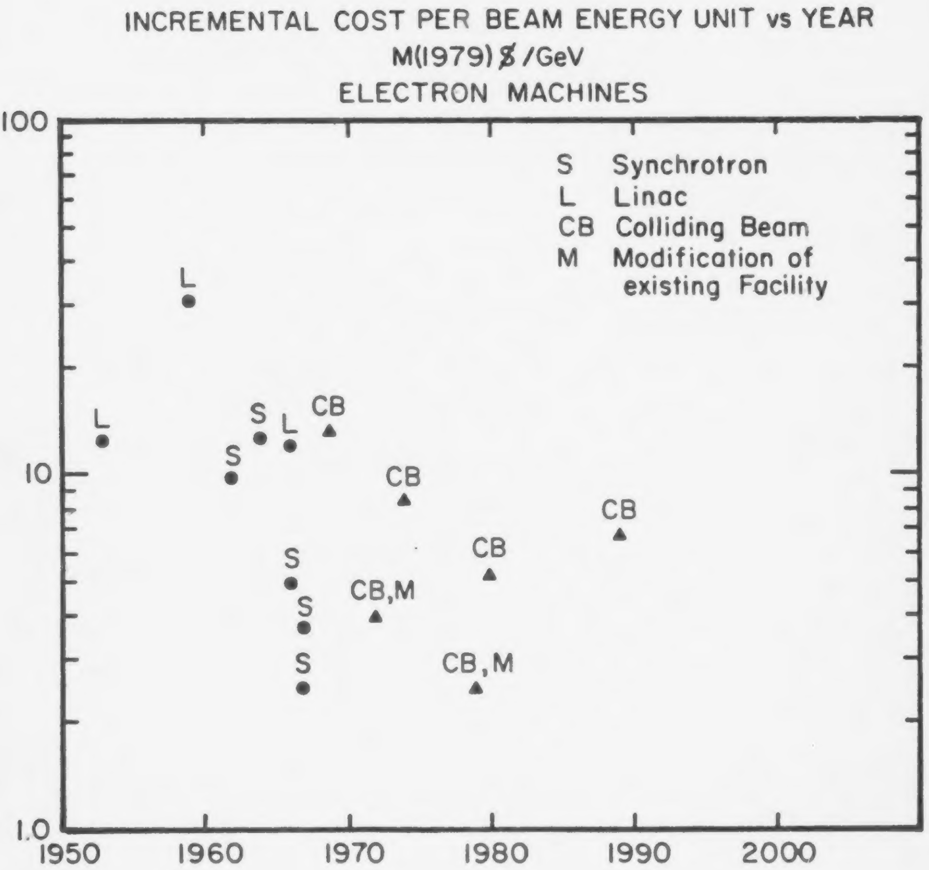
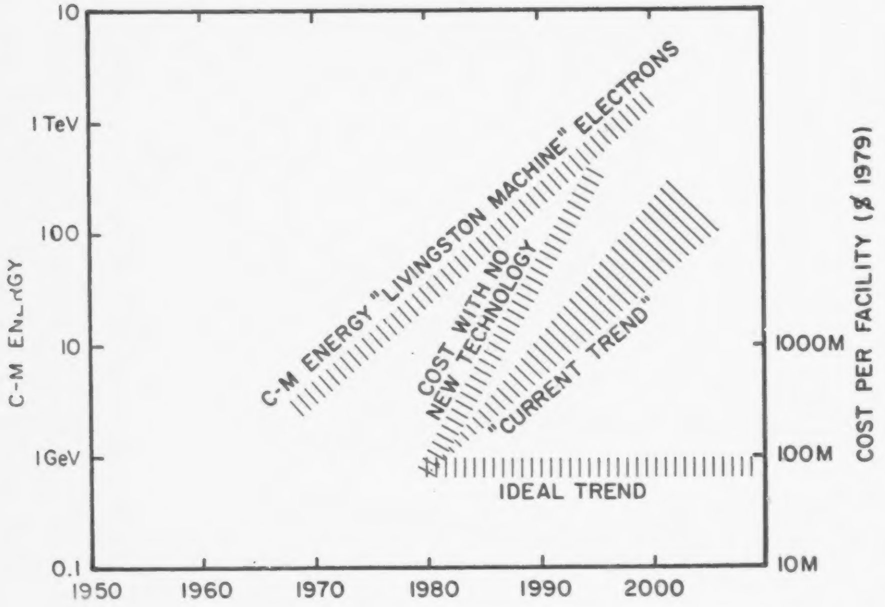

ENERGY AND COST GROWTH OF
PROTON ACCELERATORS USING THE
MOST ADVANCED TECHNOLOGY
100
10
1TeV
100
10
1GeV
C-M ENI
1950
1960
1970
1980
1990
2000
1000M
100M
10M
COST PER FACILITY ($)
C-M ENERGY OF "LIVINGSTON MACHINE" PROTONS
COSTS WITH
NO NEW TECHNOLOGY
PAST TREND CONTINUES
HISTORICAL COST
TREND
"IDEAL TREND"

Fig. 9



INCREMENTAL COST PER BEAM ENERGY UNIT vs YEAR
M(1979) $ /GeV
ELECTRON MACHINES

Fig. 10



ENERGY AND COST GROWTH OF
ELECTRON ACCELERATORS USING THE
MOST ADVANCED TECHNOLOGY

Fig. 11

accomplishing needed energy increases. Most importantly, it will be necessary for us to apply the utmost ingenuity to the reduction of operating costs, particularly electric power costs. Currently some 10% of operating resources available to the field are used to pay for power.

These then are the challenges that we face. The prospects for further unit cost improvements are difficult to assess accurately, since one cannot anticipate invention but merely try to nurture it. As in the past, however, an important component of progress will doubtless be the further development of technologies now being used or just being brought into use. Here again assessment is unsure because one cannot anticipate invention but one can make estimates as to possible limits in unit cost improvements for current technologies. For orientation, we give two rather superficial examples.

As a first example, consider the superconducting magnets which are to provide the guide fields for the next generation of proton colliders. Superconducting magnets now in development will operate at 4 to 5 Tesla. Intensive development can perhaps lead to a 10 Tesla magnet of comparable weight and complexity, or alternatively, the cost per pound of magnets of current performance can be improved. The combination of such developments might yield unit cost improvement factors of perhaps 2-3. These figures indicate that such efforts can make important contributions to cost reduction. Yet such improvements are limited and this fact highlights again the need for qualitatively new ideas if really large increases in energy are to be achieved economically.

As a second example, one can address electron accelerator systems in a similar vein for rough assessment of potential. The original SLAC machine achieved a gradient of 7 MeV/meter. By raising the peak power, this was increased to 11 MeV/meter. Further conversion will bring it to 17 MeV/meter. Preliminary experiments indicate that accelerator structures similar to those now in use may be able to sustain perhaps up to 100 MeV/meter. This high gradient can lead to reduced unit energy costs only if the costs of the required higher power RF systems become comparable with present power systems, an area where considerable work needs to be done. Superconducting RF structures also offer substantial potential. A niobium structure can support a surface magnetic field of 1.5 kilogauss, which corresponds to an effective accelerating gradient of about 40 MeV/meter. In such a machine the refrigerator is a major component, filling a role comparable to the RF power sources for a normal pulsed linac. In a superconducting linac, the cavity costs are likely to be at least as large as those of a normal linac. Therefore, to compete with the normal linac in terms of unit energy cost, the losses must be very low. Operating Q's of several x 10^{10} are required to make refrigerator costs equal to or lower than those for the RF system of a corresponding normal linac. Such Q's have been achieved in small assemblies.

While this simplistic approach does not tell the whole story by any means, it is evident that maintaining the improvement factor of 6 per decade, implied by the ideal cost curves presented above, would require that the full potential of existing or barely existing technologies be reached on the time scale of a decade. Looking to the decades beyond, we may be quite sure that we shall need strikingly different technologies. We should be laying their foundations now.

B. Balance of Short-Range, Mid-Range, and Long-Range R&D

These three categories form parts of a continuum. Their boundaries are somewhat arbitrary. We have defined R&D applied to projects (RDAP) as being activities in support of operations, current construction, and proposals aimed at starts in five years or less. As noted earlier, we are currently spending about 13% of our operating resources on RDAP, the majority of this being devoted to support of the large projects, utilzing superconducting magnets, now underway. The key question about these near- and mid-term activities is whether they are adequate to meet the construction commitments of the program. A useful answer to this question cannot be obtained without detailed review of each project in its entirety. This we have not done. We have reviewed the RDAP components of the projects in isolation from their other important components, and do not believe that current RDAP expenditures are excessive.

In view of the future challenges, detailed in previous paragraphs, we do believe that AARD (Advanced Accelerator R&D) focuses primarily on the future, is receiving insufficient attention. It is to these longer range needs that the majority of our review and recommendations are addressed.

C. Technical Areas in Long-Range R&D Now Receiving Insufficient Attention

We list here four categories among long-range R&D activities:

1. Further refinement or extension of established devices or technologies

By this we mean to include the exploration of the ultimate performance limits of the devices and technologies used in accelerators and, most importantly, thorough analysis of the devices themselves and of the accelerator systems into which they might be incorporated. Such studies are needed to reveal the important R&D problems that need solving and to evaluate the potential unit cost improvements that may be realized through the R&D. Likely candidates for such studies are superconducting magnets, low temperature helium refrigerator systems, linac structures and microwave power sources.

2. Promising methods, devices or technologies

In this category are all forms of beam-cooling methods, stochastic cooling, low energy electron beam cooling and relativistic electron beam cooling of protons. Also in this category are linear colliding beam methods, superconducting RF accelerators, and methods for obtaining very high energy polarized beams with selectable polarization direction both for electrons and protons.

3. Theoretical understanding

Each new accelerator that comes online is plagued by unexpected effects, most often collective effects, which limit its performance. Great advances have been made. The fact that colliding beam methods have yielded outstanding physics results is due to the understanding and control or elimination of several important types of instability. Nevertheless, performance is still sharply limited by poorly understood phenomena. In the case of the non-linear beam-beam effect, we are still forced to base our designs on semi-empirical formulae of unknown scaling properties and upon only qualitative understanding of the phenomena. Control or amelioration of this effect would yield benefits commensurate with substantial investments in further experimental and theoretical studies of the effect using existing accelerators. Better diagnostic methods need to be developed, and considerable beam time on existing accelerators must be allocated if there is to be any realistic expectation of advance in this area. Similar remarks apply to intensity limits in single-beam machines and storage rings. Success in these endeavors would result in both lowering of accelerator capital costs and, through the more efficient use of accelerated beam particles, a decrease in operating costs.

4. New or unexplored accelerator principles and technologies

It is evident that there is a need for ideas and techniques which, without enormous increases in complexity of apparatus, will allow qualitative increases in performance. Success here is measured in GeV/meter or per kilogram of apparatus or, ultimately, in GeV/$. One might call this parameter capital efficiency. Equally important, of course, are ideas leading to improvements in energy efficiency. Obvious candidates for attention are in short supply.

Certainly more attention and evaluation need to be focused on laser or ultra-high power mm wavelength-source-driven-linacs and linear collective effects accelerators, as well as on the power sources themselves, e.g., specially designed optical lasers, free electron lasers, cyclotron masers and the like. While it is not clear that any of the very high current, low energy collective electron beam devices now in development will have direct HEP accelerator application, they should be examined for potential in microwave generation or in beam focusing or manipulation devices.

Perhaps the most important thing at the moment is to heighten the awareness in the community that there is a real need to be met and to be open to new ideas that may emerge.

D. Communications Within the Field and With Other Fields

Communications within the accelerator community are quite good but could benefit substantially from more systematic attention. This is particularly true with regard to laboratory internal reports, the most important single communications vehicle at the present time. There is a continuing need for up-to-date books and tutorial materials. The topical workshops that have come into use in recent years have shown that, if properly focused, they can serve very efficiently both as communications media and as generators of ideas.

The principal means of communication within the field are the refereed journals, laboratory internal reports, conferences, workshops and the published proceedings of these meetings as well as books and other tutorial documents. At this time the refereed journals play a relatively minor role in the overall communications network of the accelerator field. They are important for interfield communications and do help some in the dissemination of new ideas and of synthesis and codification of information about accelerator physics and technology.

The character of the field today has brought about the wide use of less formal means of recording and dissemination, primarily laboratory internal reports and preprints. Three aspects of this medium deserve comment. Errors in such internal reports and preprints remain uncorrected and therefore such media have limited reference value. Generally, specialists already in the field find it easy to get their names on distribution lists for reports from other labs. Other younger, less well-known or more peripherally connected workers do not receive the reports directly. At some laboratories, pains are taken to collect accelerator materials sent to individuals and to display them centrally or to copy and distribute them to the appropriate intra-lab community. At other laboratories such efforts are weak or non-existent. With regard to production of such materials, some laboratories have strong traditions of reporting, judging, and disseminating their own internal accelerator work. Such activities are clearly of benefit to the laboratory itself and to the community at large. Other laboratories do not have such a strong tradition.

The system of topical and general accelerator conferences, both national and international, works quite well and deserves the continuing support and participation of the HEP accelerator community.

Because of the relatively small size and specialized nature of the high energy accelerator field, the creation of suitable books covering introductory through advanced topics has been difficult. The few books that have been produced are widely used and have had an enormous impact on the field, both through codifying existing knowledge into a readily understood form and through the establishment of a common terminology. More books covering recent advanced are needed.

Communication with other fields varies from strong to weak. The widespread and effective network of the IEEE as well as commercially oriented channels keep the field in rather good contact with electrical and electronic engineering. Similar, if somewhat weaker statements can be made with regard to other branches of engineering science, such as mechanical engineering, cryogenics, vacuum technology and materials, although it is perhaps true that more effort in keeping abreast of materials technologies, particularly with regard to cryogenic behavior of metals and plastics and of advances in adhesives, may become necessary.

Of more direct relevance to accelerator physics itself is the field of plasma physics. Here lines of communication are established through persons who participate in both fields and through scientific journals. Despite such important links, most accelerator physicists are largely ignorant of the powerful methods and insights that have been developed to deal with collective phenomena by plasma specialists and vice versa. We need to find ways to educate ourselves in the language of plasma physics so that we can adapt relevant work in that field to our needs. Clearly, two-way communication would be of benefit to both fields.

E. Manpower Resources and Training

From the data available to us, presented in Section IV.E, it appears that the manpower supply is quite elastic but that the age structure of the participants in accelerator work tends to be somewhat older than that of other healthy fields. The data indicate that many workers enter the field without special formal schooling in accelerator science. This means that our accelerator physicists and engineers are nearly all trained on the job. Currently, in the U.S., most of them come with backgrounds in high energy physics with a few from engineering and plasma physics. A qualitative change in this situation is not likely to come about. What is possible and desirable is a more systematic training effort within the institutions doing accelerator R&D, both national laboratories and universities. This could be aided by compilations of special tutorial materials, the supporting of seminar series, and the holding of summer schools aimed at persons with various levels of experience in accelerator work.

F. Decision-making Processes and Resource Allocation

As previously noted, our decision-making machinery for R&D work is characterized by diversity. We firmly believe this to be healthy and necessary. Within the totality of present mechanisms we have the capability to foster and support an adequate long-range R&D effort. What seems to be lacking is a strong enough perception of need within the community. Such a perception of need is necessary for two major reasons. First, it furnishes the moral support necessary for the various actors in the decision-making chain to make the difficult decisions required in making resources available for the work. In this chain we include user groups, in-house HEP research staffs, laboratory advisory bodies and laboratory directorates, as well as the agencies and Congressional Committees. Second, and of equal importance, a heightened perception of need can result in a more active participation by both HEP theorists and experimentalists.

The recommendation implied by these observations, namely that we tax the short-range program for the sake of assuring the health of the field in the rather far future is, of course, very difficult to follow. For this reason, the community needs to be assured that work undertaken is purposeful and of the highest quality.

VIII. CONCLUSIONS AND RECOMMENDATIONS

Accelerator Research and Development has always been an integral and important part of elementary particle physics research. The physics output has been paced, to a large extent, by the improvement in performance of our accelerators. In the past 50 years, equivalent beam energies available to experimenters have increased by a factor of ten million as a result of accelerator R&D efforts. Not only performance increased exponentially but unit costs have also been reduced significantly. Even in times of decreased real spending power for the field, significant energy increases were achieved.

Despite our remarkable achievements in unit cost reductions, these reductions have not kept pace with the energy increases achieved. This has resulted in a limitation in the number and diversity of high energy accelerator facilities available to workers in the field. Further decreases in diversity would be necessary to continue frontier research were the pace of cost reduction produced by new accelerator technologies not to increase. This would have a serious negative impact on the scientific productivity of U.S. elementary particle physics. The challenge is clear: we must redouble our efforts to reduce unit costs sharply while maintaining performance improvements through full exploitation of the potential of our current accelerator technologies and through invention of entirely new technologies and methods.

Historically, we have spent approximately 10 percent of our operating resources on accelerator R&D activities. This includes both R&D applied to projects (RDAP) and long-range R&D (AARD) which is not associated with a specific project. Today this fraction for all accelerator R&D is 14%, reflecting our heavy commitment to superconducting magnet development. The great majority of this RDAP is, as it needs to be. The Subpanel has determined that between 1% and 1 1/2% of operating resources are now devoted to AARD. If we are to meet the challenge of the future, more of our resources need to be devoted to this activity. We believe that an appropriate level for long range R&D would be about 4% of HEP operating resources. In arriving at this judgment we have considered both the desirable expansion of efforts now underway and the need for new efforts whose costs we have estimated from experience in related accelerator research. The following table gives a reasonable example of a program operating at the 4% level. It is meant as an illustrative example not as a specific recommendation of the Subpanel. Nevertheless, a variety of such exercises has convinced the Subpanel that a significant attack on the long range problems requires an investment of about this level.

ILLUSTRATIVE FUNDING MODEL FOR ADVANCED ACCELERATOR RESEARCH AND DEVELOPMENT (AARD)

	Activity	Current Level (M$) in HEP Program		Proposed Level (M$)	Comments
1.	R&D on "next generation" superconducting magnets at the approximately 10 T level	1.0	.8 LBL .2 BNL	2.5	These magnets will almost certainly be used in future facilities and cannot be built without substantial research into their basic physics and without development of engineering solutions to the difficult materials and structural problems involved.
2.	R&D on RF superconductivity	.8	.4 SLAC .4 Cornell	1.5	Superconducting RF cavities are a very promising approach to the problem of increasing the energies of both circular and linear electron accelerators. However, fundamental research remains to be done before their potential can be realized.
3.	Studies of fundamental accelerator phenomena such as performance limiting phenomena, cooling, etc.	.9	AGS FNAL SPEAR CESR	1.5	These studies are needed to optimize the design of any new accelerator and will be increasingly important to future accelerator where the most economical and tightly optimized design will be a key factor in the energy and performance achieved.

ILLUSTRATIVE FUNDING MODEL FOR ADVANCED ACCELERATOR
RESEARCH AND DEVELOPMENT (AARD)
(continued)

Activity	Current Level (M$) in HEP Program	Proposed Level (M$)	Comments
4. Theoretical and experimental studies of new accelerator concepts	0.2	2.5	All of our current understanding indicates that new approaches will be needed if we are to continue to move to higher energies beyond the limits we can foresee for the technologies we currently know about. Some possibilities exist and we believe several should involve a substantial experimental program at any given time. By analogy with the non-HEP accelerator projects noted in Appendix I we estimate a typical annual cost of 1 M$ for each such undertaking.
5. Other, e.g., new power sources, beam manipulation devices and beam diagnostic equipment	---	1.0	---
TOTAL	2.9	9.0	

The creation of an actual augmented program should follow the established pattern of initiative and review.

It is obvious that no level of expenditure for AARD can guarantee that the historical trend in unit cost reduction can continue. Nevertheless, the Subpanel is convinced that without increased effort on AARD significant unit cost reduction is most unlikely. Thus, we conclude that increased effort on AARD is an essential investment for the community at this time.

Recommendations

1. The high energy physics community should significantly increase its support for AARD. The Subpanel estimates, from an examination of significant areas of this research, that the level for AARD in the HEP accelerator field should be increased from about 1% up to about 4% of current operating resources. This increase should be achieved within the existing administrative framework.

2. In reviewing accelerator R&D, the Subpanel has identified certain specific technical areas that should be emphasized in AARD. This list is not presumed to be complete, exclusive, or to indicate relative priorities. Rather, it is intended to be an indication of the challenges of AARD and to underscore the importance of AARD to the field of high energy physics. This list of specific topics is:

   (a) Development of very high field accelerator magnets and the evaluation of the practical limits of this technology. In view of the large scale of this enterprise and its uniqueness, the Subpanel recommends that this AARD effort be carried out as a collaborative effort among the laboratories having capability in this area. The Subpanel feels that this development of high-field magnets should be focused toward a specific accelerator goal.

   (b) Development of liquid helium refrigerator systems with goals of improving efficiency and reliability and providing operation at reduced temperatures.

   (c) Theoretical and experimental exploration of the limits to microwave linac gradients and to the peak powers that can be delivered in the S- to X-band regions.

   (d) Basic physics and device development in superconducting RF accelerators.

   (e) Theoretical and experimental studies of basic accelerator phenomena, particularly the beam-beam interaction and other performance-limiting phenomena.

(f) Search for and preliminary development of new accelerator schemes with high performance potential such as laser accelerators or other devices using ultra-high peak power with or without collective effects.

(g) New techniques and devices for manipulating very high power and/or very high energy beams.

(h) The general problem of increasing the brightness of particle beams with emphasis on cooling high energy beams.

(i) Development of new beam diagnostic techniques and devices.

3. Laboratory and University managements, aided by the agencies, should take specific measures to make participation in accelerator R&D practical and more desirable for high energy physics theorists and experimentalists not now so engaged. This could include payments of salary for sabbatical leaves and leaves of absence, and on-the-job training through seminars, summer schools and the use of specially assembled tutorial and review materials. Also expansion of university-labortory collaborations could be included.

4. Laboratory and University managements, aided by the agencies, should take specific measures to facilitate cross-fertilization from other fields such as Plasma Physics, Lasers and Materials Science, and other accelerator-related activities. This could include long-term visits by outstanding individuals in these fields for the purpose of working directly with laboratory staffs, giving tutorial seminars or lectures series, or participating in extended topical workshops.

APPENDIX I

R&D ON ACCELERATORS TO PRODUCE VERY HIGH BEAM CURRENT

I. INTRODUCTION

II. CATEGORY 1: NON-COLLECTIVE SYSTEMS

1. Ion Accelerators
2. Electron Induction Linacs

III. CATEGORY 2: COLLECTIVE DEVICES AND CONCEPTS

1. Transverse Focussing
2. Longitudinal Acceleration
3. Wave Accelerators

IV. SUMMARY AND CONCLUSIONS

V. ACKNOWLEDGEMENTS

VI. SELECTED REFERENCES

I. INTRODUCTION

Apart from traditional accelerator research in support of present and proposed accelerators for High Energy and Nuclear Physics, there is a considerable body of active research, largely supported by DOD funds, on accelerating devices and concepts of a novel nature. A selection of these will be reviewed below. Programmatically, these activities span a broad range of risk/benefit ratios ranging from extension of existing parameters for accelerators with well-established principles of operation to exploring speculative concepts that may in the future provide a new tool for accelerator physicists to deploy in constructing novel types of accelerators or expanding the capability of more traditional machines. Likewise, there is a broad spectrum of funding levels ranging from a wide variety of exploratory programs (at greater than 100 K$/Year level) through a small number of selected well-supported programs (at greater than 1 M$/Year level), to a single significantly large construction program (ATA at 10 M$/Year level). In general, all these programs are concerned with producing very high beam current in a pulsed mode and with the potential for later being scaled to large beam energy (joules not MeV).

It is important to note that attention in all these programs has been focussed on producing beams with kinetic energies less than about 1 GeV, and it will be seen that a rich variety of novel ideas and tools has been developed. Hitherto, there has been almost no stimulus to address, in a similar way, the applications of some of these concepts to ultra-relativistic particle beams for high energy physics applications. Given such a stimulus it could well be that the ingenuity which has been brought to bear on solving the medium energy high-current electron and proton beam problems could well in the future lead to novel advances in the art of High Energy Physics particle beams.

Broadly speaking, the programs to be discussed fall into two categories:

1. Non-Collective Systems

These include rf linacs (for ions) and a variety of induction linacs (for electrons); here one relies on externally produced ("conventional") electric and magnetic fields for focussing and acceleration.

2. Collective Devices and Concepts

This category can be sub-divided into cases where the collective electric field is primarily being used:

(a) Radially (E_r) to provide transverse focussing of intense ion beams (protons or electrons); or

(b) Axially (E_z) to provide very strong accelerating fields for ions (protons); in all such cases the radial component of the electric field, E_r, also acts to constrain the beam laterally.

It is convenient to distinguish between these broad categories in the following way. If one ignores the charge on the particles in the accelerator beam itself then at the location of the beam we have for Category 1:

$$\text{Div } E = 0, \text{ and Curl } B = 0 \qquad \text{(Eq. 1)}$$

and for Category 2:

$$\text{Div } E = \text{rho, and, sometimes, Curl } B = j \qquad \text{(Eq. 2)}$$

In the one case the fields are produced by external conductors and have constraints set by the mechanical and surface properties of solid materials; in the other case, charged particles (electrons and/or ions) are present in free space at or near the beam location to provide fields that are not subject to the conventional boundary conditions either mechanical or electrical. It is this latter feature that has led to the very wide variety of collective effect devices that has been suggested. The other side of [illegible] that often the electron beam used to manipulate the intense [illegible] of even higher intensity and must itself be amenable to outside control and regulation.

II. CATEGORY 1: NON-COLLECTIVE SYSTEMS

Devices that fall under this heading include a high-current rf linac, a high current storage ring and a variety of induction linacs.

1. Ion Accelerators

(i) High brightness rf linac (AT-2, T. Hayward, LASL): The aim of this program is to produce a C.W. beam of 100 mA of H^- ions of very small emittance (2pi x 10^{-7} meter-radians) from an rf linac accelerator. The goal of the first test-stand experiment is to accelerate the beam to 4.5 MeV. A high-quality 100 mA beam, i.e., of satisfactory emittance, of $\overline{H}$ ions has been successfully generated from a pulsed (20 msec) Dimov-Dudnikov source. A rotating version of this source which will allow for heat dissipation and permit d.c. operation is being developed.

This beam will be accelerated through a 200 kV electrostatic column and it remains to be seen if the beam-emittance at the exit continues to be satisfactory. The beam will then be bunched and injected into an rf linac operating at 400 MHz. In the first sections of the linac the emittance is expected to grow because of coupling of the transverse and longitudinal motion of the presence of collective self-fields of the intense beam. Extensive numerical simulation calculations of the emittance dilution are underway. An elegant laser diagnostic to study this coupling experimentally will sample different points within each rf bunch; the focussed laser light will convert a tiny volume of the negative ions to neutral atoms which can stream out of the machine where their angular divergence and energy spread will be measured. The transverse and longitudinal emittance changes can thus be mapped out cell by cell along the length of the first linac section. Emittance dilution in the front end of rf linacs has been known to exist for many years, this experiment looks as if it should lead to an excellent understanding of the physical processes at work.

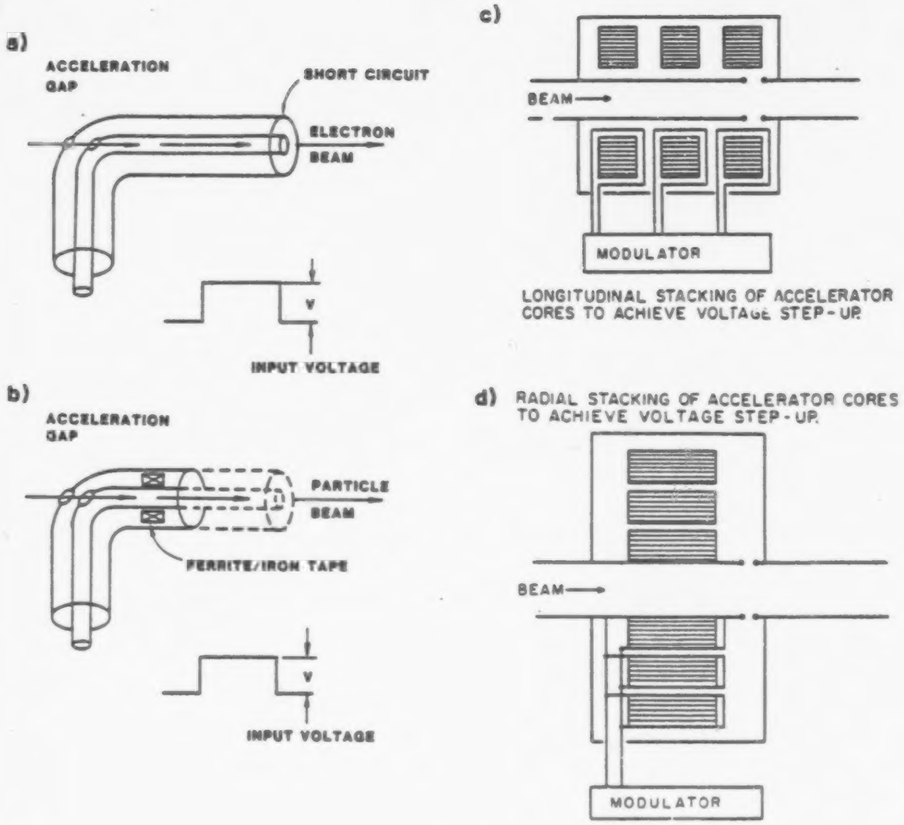
This program provides a good example of research in one area having spin-off into another -- in this case HEP. It was during the course of this research that the samarium-cobalt permanent magnet quadrupole, which can provide ultra-high gradients for focussing very narrow beams, was invented. It is now perceived that such quadrupoles can provide a crucial capability for handling the microscopic e^+ and e^- beams at the intersection region in the proposed single-pass collider at SLAC.

(ii) High-current storage ring (AT-3, G. Lawrence, LASL): The design is underway for a storage ring to store multiple turns from the LAMPF 800 MeV proton linac. The goal is to produce an external bunched beam of 1000 amperes peak current. Numerical simulation codes have been used to understand the injection process and the beam stability for large circulating beam current. Destructive collective self-field effects must be carefully avoided at the final current level.

2. Electron Induction Linacs

An induction linac is made up of a succession of small pulse-power modules each time to give an energy increment to the particles at the moment of passage of the beam. In typical rf modulators the peak power capacity exceeds the C.W. power rating by perhaps a factor of ten; in pulse power devices the corresponding factor can be about 10^5. Since, therefore, gigawatts of power can be supplied per module the induction linac is ideally suited (and very efficient) for accelerating very large peak beam currents (100-100,000 Amps.). A Marx generator, usually, is used to charge a pulse forming network or transmission line. The geometry of the line can be arranged so that voltage of only one polarity (accelerating) is seen by the beam. For short beam pulses a vacuum or dielectric line can be used provided the double-transit time is adequately long; for long pulses a high impedance termination (ferromagnetic toroid) is used to exclude the unwanted polarity from the beam for as long as it takes the magnetic material to saturate. (See Figure 1.)

(i) Advanced Technology Accelerator (ATA, R. Briggs, LLL): At present under construction this 50 MeV ferrite-loaded linac is intended to deliver 10,000 amps of electrons in 50 nsec pulses. The average repetition rate is 5 Hz with a burst mode capability of 1 kHz for 5 pulses. Water-filled Blumleins are used for the pulse-forming lines. The 5 MeV gun (ETA) has been completed and has so far delivered 80% of the design current at the desired repetition rate. The development of reliable high-voltage (250 kV) spark-gap switches to operate at 1,000 times per second was a significant technological advance.


a)
ACCELERATION GAP
SHORT CIRCUIT
ELECTRON BEAM
V
INPUT VOLTAGE
b)
ACCELERATION GAP
PARTICLE BEAM
FERRITE/IRON TAPE
V
INPUT VOLTAGE
c)
BEAM
MODULATOR
LONGITUDINAL STACKING OF ACCELERATOR CORES TO ACHIEVE VOLTAGE STEP-UP.
d) RADIAL STACKING OF ACCELERATOR CORES TO ACHIEVE VOLTAGE STEP-UP.
BEAM
MODULATOR

XBL 804-9241A

Fig. 1: Evolution of the induction linac geometry. In (a) it can be seen that a shorted transmission line with a hollow center conductor can accelerate particles across the gap shown; the voltage disappears, however, when the inverted reflection returns, from the shorted end to the gap. For pulses much longer than a few nanoseconds this would provide a very low accelerating gradient. In (b) ferromagnetic material increases the electrical line-length and thus allows long pulse-length without sacrificing gradient. Several cores driven in parallel can provide increased gap voltage: they may be stacked axially (c) or radially (d). The latter was the choice for the NBS 2 μsec induction linac.

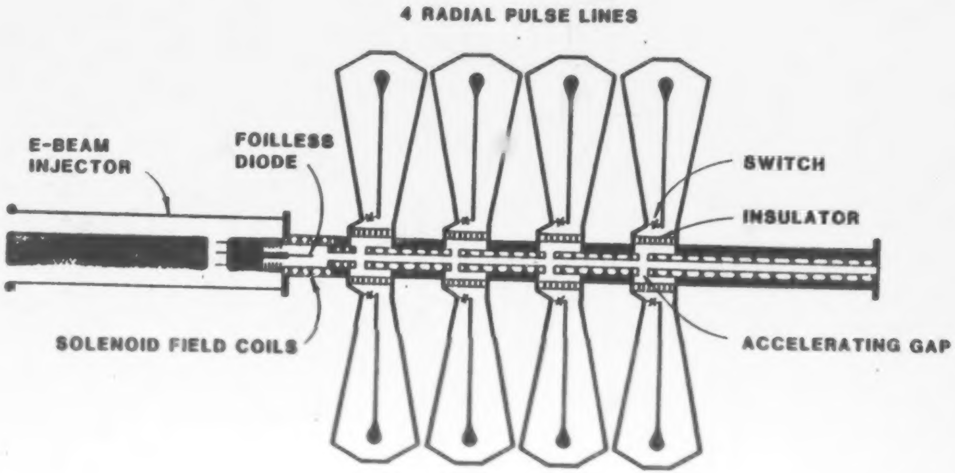
(ii) Long pulse induction linac (S. Penner, NBS): For pulse durations much longer than 100 nsec ferrite becomes unduly expensive. The NBS program, which is now winding down, addressed the problem of using thin (one mil) inexpensive iron sheet, insulated layer to layer, as a core material suitable for a pulse duration of 2 microsec. In addition, this design included the novel feature of stacking several (n) nested ferromagnetic toroids of successively larger radii. These can all be driven in parallel from a single pulse line (of voltage V) and so an accelerating voltage nV can be developed across a single gap. Such an arrangement leads to a reduction in the overall length of the accelerator at the expense of a more bulky transverse dimension. (See Figure 1(d).)

The NBS machine was operated at 018 MeV and 1000 amps electron beam current. Experiments with this beam gave a striking demonstration that a gas-focussed beam can propagate for long distances in low pressure gas (1-30 Torr) and can even be bent through 360° with dipole magnets only (no lenses, only gas-focussing) for recirculation through the accelerating cavity. This has been done to give a final beam energy of 1.2 MeV.

(iii) Radial line accelerator (RADLAC, B. Miller, AFWL/SA): For short pulses (about 20 nsec) the pulse-forming line can be a radial transmission line closed at the outer radius and, if one wishes to minimize the transverse size, it can be filled with dielectric. Such a design was first published in the USSR by Pavlovskij. The RADLAC consists of 4 such radial lines each supplying 2 MV across a two-inch gap. By use of a 2 MV pulse-power (PFN plus diode) injector the final performance is intended to be acceleration to 10 MeV of a 50 kA electron beam with a pulse length of 15 nsec.

If one analyzes a transmission line initially charged to voltage, V, which is suddenly shorted by a fast switch at one end, one finds the following voltage behavior at the open-circuit end. The voltage remains at the value V for a single transit-time, tau, after switch closure; the propagating wave, -V in amplitude, initiated by the short then arrives and reflects at the open-circuit with the voltage doubling to -2V. The resultant voltage amplitude is (V-2V) = -V which persists for a double-transit time, 2tau, by which time the pulse has returned from the shorted end, and is now inverted to +V. Thus it can be seen that, in the absence of losses, the output voltage will be a train of square pulses each 2tau long and alternating in amplitude from +V and -V. The only exceptional pulse is the first one which is only tau in length. For acceleration one can choose to use either the first pulse or the second if a longer pulse length is desired.

Figure 2 shows how the radial lines are arranged in the RADLAC. Each consists of a flat inner conductor flanked on either side by slightly conical outer conductors to form a tapered line of constant impedance (10 ohms). It is a "folded" geometry with both the switch and gap located at the inner radius. The oil-filled cavities are 3 meters in diameter and of fairly simple sheet-metal construction. A circular hole in the center is surrounded by a graded insulator (which provides the oil-vacuum envelope) and allows for passage of the beam. Arranged symmetrically around the cylindrical insulator are eight self-closing oil switches which fire in synchronism as the potential on the inner conductor is brought up rapidly. During the passage of the beam no field is present on the (shorted) switch side of the line while the other side acts as an accelerating


4 RADIAL PULSE LINES
E-BEAM INJECTOR
FOILLESS DIODE
SWITCH
INSULATOR
SOLENOID FIELD COILS
ACCELERATING GAP

XBL 804-9240A

Fig. 2: The RADLAC comprises four folded radial pulse lines which are filled with oil. The taper is chosen to provide constant characteristic impedance in the radial direction. The 2 MeV injector is an E-beam generator and diode.

gap. Solenoid lenses provide magnetic focussing. As of early 1980, the injector and 4 cavities were operating to produce a 20 kA beam at 9.5 MeV.

(iv) Auto-accelerator (M. Friedman, NRL): This program is an ingenious effort to exploit the high-current electron-beam technology that has been developed in the sub-10 MeV region to produce electron-beams at very much higher energies, perhaps 100 MeV. In contrast to the RADLAC geometry the cavities have their long dimension (1m) in the axial, not the radial, direction (Figure 3). Each cavity acts as a transmission line with a double transit time, 2 tau = 6 nsec. The mode of operation is highly novel; an intense electron beam passing through the pipe is arranged to charge the cavities with magnetic energy on a slow time scale and this energy is later extracted quickly, in a double-transit time, to accelerate a 6-nsec pulse of electrons near the tail of the beam pulse.

The relativistic electron "charging" beam rises linearly from zero to I=30 kA in a time of 800 nsec. The beam current i(t) acts as a current source for the transmission line and instantaneously contributes a voltage, $Z_0 i(t)$, at the gap. If one follows how each such signal increment reflects back and forth along the line with inversion at the shorted end, a doubling at its first return to the open-circuit end and, in the absence of losses, repeated reflections thereafter of alternating sign, one can synthesize the voltage waveform developed across the gap. This turns out to be a linear rise to a value $Z_0 i$(2tau) followed by a linear fall to zero at t = 4tau and a repetition of this triangular form as long as the current rise continues. Thus the average value of the gap voltage is 1/2 $Z_0 i$(2tau). We find that this is equal to Lc(tau)di/dt where Lc(tau)is the lumped-element (long time-scale) inductance of the coaxial cavity. The sign of this voltage is such as to provide a slight deceleration of the electron beam.

If the current rise is halted at i(t) = I and the electron-beam current returned to zero the destructive reflections that keep the gap voltage at this low value are suddenly removed and it can quickly be verified that a large accelerating voltage, $Z_0 I$, appears on the gap for a time, 2tau. In the NRL auto-accelerator the electron-beam current is switched not to zero but to 0.2I, so that the accelerating voltage per gap is 0.8 $Z_0 I$. (See Figure 3.)

What is distinctive about this device is that it circumvents two of the major problems of pulse-power accelerators -- the switches and the insulators. Since the magnetic energy release from a cavity begins just when the downward step in beam-current occurs at the gap location, the accelerator is automatically self-synchronized from gap to gap; jitter is eliminated since switches are not needed. Insulators at the acceleration gaps are also not required; for short pulses very high voltages (3 MV) can be achieved across just a few centimeters in vacuum. Finally, the accelerator can be designed to have relatively high-efficiency from the wall-plug to the beam, perhaps in the renge of 30%.

In the experiments now in progress, the injector is an E-beam generator, with a transmission-line for pulse forming, which produces a 30 kA, 1.5 MeV, hollow beam from a foiless diode. This beam is transported in a uniform-field solenoid

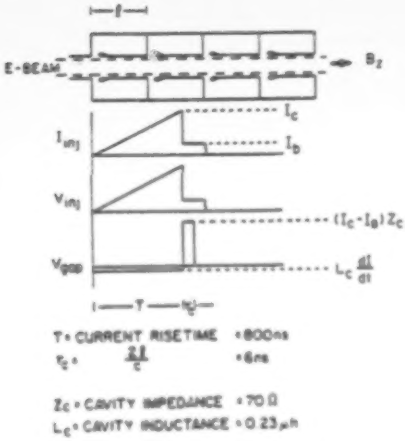

ℓ
E-BEAM
B_z
I_c
I_{inj}
I_b
V_{inj}
$(I_c - I_B)Z_c$
V_{gap}
$L_c \frac{di}{dt}$
T
τ_c
T = CURRENT RISETIME = 800ns
$\tau_c = \frac{2\ell}{c}$ = 6ns
Z_c = CAVITY IMPEDANCE = 70 Ω
L_c = CAVITY INDUCTANCE = 0.23 μh

Fig. 3: The NRL autoaccelerator concept. From top to bottom the figures show: Cavity structure; injected current, i(t); voltage developed across each gap showing the time-averaged retarding voltage, $L_c \frac{di}{dt}$, during the current rise, and the accelerating voltage during the current drop.

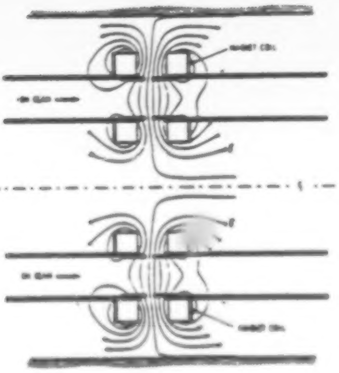


Fig. 4: The arrangement of the four field-coils to produce the desired magnetic field in an accelerating gap of the Pulselac. Note that the ions form a hollow cylindrical beam situated in the space between the two coaxial conductors that make up a drift tube.

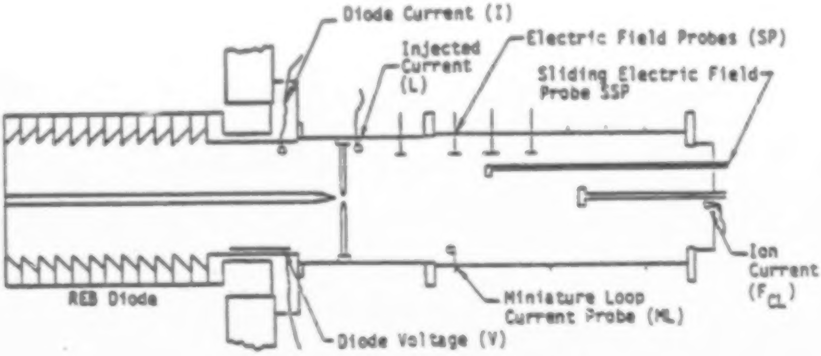

Diode Current (I)
Injected Current (L)
Electric Field Probes (SP)
Sliding Electric Field Probe SSP
Ion Current (F_{CL})
REB Diode
Miniature Loop Current Probe (ML)
Diode Voltage (V)

Fig. 5: Schematic of a Luce diode in a form used at Boeing. The pointed cathode is pulsed to high voltage by a pulse-power E-beam generator. Monitoring and diagnostic probes are labelled.

magnet (15 kG) through a sequence of coaxial cavities. Six cavities are planned for the proof-of-principle experiment; as of early 1980 two had been installed and successfully tested. Electrons were accelerated from 0.3 MV to 3 MV with 4 kA beam current. Some "cross-talk" was encountered between the two cavities but was eliminated by reducing the Q-factor of the cavities.

It is tempting to call this device a collective accelerator in which electrons are used to accelerate other electrons but it does not comply with the spirit of the definitions in the introduction in that the electromagnetic field occurs as an intermediary between the action of one set of electrons and the reaction of the other set. (Note, for comparison, that in a conventional rf accelerator electron-beam tubes create rf fields that are coupled via wires or waveguides to cavities and thence to the beam.)

III. CATEGORY 2: COLLECTIVE DEVICES AND CONCEPTS

1. Collective Effects for Transverse Focussing

(i) Some examples of proven utility: The transport of an ion beam from the ion-source to the Cockcroft-Walton column in a standard accelerator pre-injector poses serious problems for very high intensities and for heavier ions; these arise from limitations on the strength of magnetic lenses. Several examples are now known (e.g., 60 mA Xe source at LBL, 150 mA Deuteron RTNS at LLL) where a suitably "gassy" condition of the transport line can be arranged so that ionization is caused by the head of the beam, the cold ions drift to the walls of the beam-pipe, and the electrons remain held in the space charge potential of the following parts of the beam. One can speak of "neutralization" of the self-field of the ion beam but in the spirit of the earlier definition in Eq. (2) the trapped electrons form a collective distributed lens. Instabilities can occur if there is noise in the plasma source with a period close to the ionization time or the cold-ion drift-time. Nonetheless, totally stable operation is easily achieved in which the electrons are relied upon to do as much as 97% of the focussing. For short pulses of ions, electron focussing has also been achieved at LBL by arranging for the injection of electrons from a grid; time dependent effects are still unexplored.

The universe situation has also been observed for electron beams when some gas is present; here the cold ions remain for focussing and the cold electrons are expelled. Stable operation under these conditions has, for example, occurred in the collector for the intense electron beam used for present cooling experiments at FNAL, and in the propagation of E-beams (LLL, NBS).

A more complicated collective mechanism enters into the stable propagation in the ballistic mode of high-current ion beams. Here the beam must be both charge-neutralized and current-neutralized, thus not alone does $\mathrm{Div}\ E \neq 0$ hold but also $\mathrm{Curl}\ B \neq 0$ if we treat that part due to the plasma current and ignore the beam. Again instabilities (e.g., kink modes) can occur but there is a window of stability.

When an ion or electron lens created by neutralization is used as a major contributor to the focussing it is usual that the beam charge density and cold ion/electron density are close to being equal. An example of a very useful collective lens in which the electron density greatly exceeds the beam ion density is due to Gabor (1947). The electrons are trapped in a cylinder with an axial magnetic field and with electrostatic reflectors at the ends. In the steady state, the surface of the magnetic flux-tube concentric with the axis is an electrical equipotential and an important feature is the possibility of controlling the value of these potentials (and hence the aberrations) by properly biasing boundary rings through which the flux-surfaces pass. Electrons can either be injected from a filament or else created by ionization of some injected gas. The lens behavior is found to be remarkably linear (BNL).

(ii) Magnetically-insulated electron-focussed ion linac (PULSELAC, S. Humphries, SA): Results to date from this program are very promising. The basic acceleration scheme is a conventional one using pulsed drift tubes to accelerate a long slug of ions. Ions are accelerated into a drift tube and when the head of the beam reaches the downstream end the voltage is removed from the drift-tube and the succeeding one switched on. Instead of using conventional focussing Humphries has arranged to inject electrons into the drift tubes to provide transverse focussing of the ion beam -- a convenient arrangement is an array of field emission points. The key feature of the scheme, however, is to prevent the electrons from crossing the accelerating gap between successive drift-tubes or they would constitute an inordinate drain on the power supply. This is accomplished by magnetic insulation whereby a magnetic field is applied in such a direction that the electrons perform magnetron orbits (with an E x B drift) but can never cross the gap and so drain the voltage generator. Obviously, fresh electrons must be injected into successive drift tubes.

Creating such a situation requires the drift tube to consist of two concentric tubes with an annular ion-beam contained between them (Figure 4). Conductors wound around the outer radius at the tips of the outer tube, and around the inner radius of the inner tube can provide a magnetic field to meet the requirement of magnetic insulation. A useful feature of this arrangement is that the E x B drift can carry the electrons around the axis again and again; thus charge-accumulation, which can be troublesome in other geometries, is avoided.

A set of plasma guns arranged in an annulus supplies about 3,000 to 4,000 amps of carbon ions for injection; a five-gap pulsed drift-tube system is now in operation that produces at its exit an impressive 3000 amps of carbon ions at an energy of 600 keV, with good emittance. These results seem to indicate that the mobile electron species can adjust its distribution in a benign way to provide focussing that is both strong and, as far as one can judge, linear.

(iii) Toroidal electron focussed ion accelerator (N. Rostoker, UCI/Maxwell): This concept is closely related to an early proposal (1956) by Budker to use an intense beam of electrons to provide a strong guide-field for protons in a circular accelerator. Several novel features are included, however, which make the idea seem attractive. The basic idea is to create a bumpy toroidal magnetic field, i.e., a string of mirrors that closes on itself, and to inject an intense cloud of electrons with predominantly transverse velocities (i.e., no toroidal component). The electrons form a deep potential well into which ions are injected; the ions are then accelerated by pulsing a transformer exactly as in a betatron. A design has been proposed by Maxwell Labs that would operate with transverse collective focussing fields of 10 MV/cm and would accelerate an extremely intense proton beam to 2.6 GeV.

The key to the operation is the local trapping of the electrons in the multiplicity of mirrors. When the induced electric field is created essentially no electrons are accelerated because the loss-cone is unpopulated. This has been verified in a small experiment at UCI. Suppression of the toroidal electron current is essential to avoid taking all the energy from the generator. The design draws heavily on results from an early collective device (HIPAC) at Avco Everett in which the technique for injection of electrons with high transverse energy was developed. Also, work on HIPAC (which was proposed by Bethe and others) succeeded in mapping out the regions of potential instabilities (diocotron, magnetron, ion-resonance) and, as a result of that work, the proposed design pays careful attention to avoiding these hazards.

A table-top experiment is underway to demonstrate proton acceleration to a few MeV. The electron guiding field will be about 1 cm in minor, and 1 m in major diameter. So far, collective focussing fields of 500 kV/cm have been achieved. Also of interest in this program is a proposal to use a plasma gun as a collective effect extraction system.

2. Collective Effects for Longitudinal Acceleration

(a) Electron Cluster Containing Ions (ERA)

This concept aims to accelerate an intense cluster of electrons which contains captured in its electrostatic well a relatively small number of ions. The lighter electrons respond rapidly to an external accelerating electric field and the protons going along for the ride thus gain energy at a rate which is enhanced by the very large factor of the mass ratio (1836 times). Since it is essential to maintain the stability of the electron cluster, Veksler proposed the creation of a relativistic electron ring which, indeed, can be stabilized. Relativistic effects for this configuration reduced the energy enhancement factor to about 40; even so, this would still be an exciting gain if achieved.

An extensive series of measurements was carried out at LBL, MPIPP (Garching) and Maryland, and a variety of instability limits (resistive-wall, negative mass, ion-resonance) identified. The potential peak accelerating rate was determined to be no more than 80 MeV/m; in practice only a few MeV/m was ever demonstrated. This work is no longer pursued in the U.S. or Germany although a very large group is continuing their program at Dubna. The Russian group has regularly accelerated protons to a few MeV and Xenon ions to over 300 MeV.

(b) Moving Potential-Well

There is a long list of experiments in this category that have successfully produced large intensities of accelerated protons and other ions. The highest fluxes of protons usually are concentrated at a kinetic energy of about three times the electron beam energy although a few mechanisms have been found to give much higher energies (greater than 60 MeV). These experiments all employ E-beam technology in which a generator -- typically a Marx generator charging a coaxial or Blumlein line -- is connected to a pulsed E-beam diode. Fairly common parameters for the electron beams are:

Energy	1-10 MeV
Current	10-100 kA
Pulse Length	few x 100 nsec.

The electron beam passes through the anode foil and enters a drift-pipe which is usually (but not always) surrounded by a solenoid to provide a transverse focussing field.

Most of these experiments rely on creating a virtual cathode within the drift-pipe and arranging for it or the head of the beam to move relatively slowly down the pipe. Background ions from the walls or a gas are accelerated by this moving potential well typically to speeds of about 0.1c. Some experiments rely on controlling a potential well by external means.

A virtual cathode will form in a metal pipe just downstream of the anode foil if the injected beam current exceeds the so-called "limiting current" I_L. Another critical current of interest is the Alfven-Lawson current limit, which is usually significantly larger than I_L.

The trick that is used is to arrange the initial parameters such that the limiting current is below the injected E-beam current to guarantee formation of a virtual cathode by the entering portion of the beam. Next one arranges to vary one or more parameters to bring I_L up to the beam current and so attain propagation; if this can take place slowly enough at first background ions are trapped and accelerated.

The general features of this acceleration mechanism were elucidated in experiments at Physics International (Putman), AFWL (Miller), SA (Olson). More recent work with gas gradients has been done at UCI (Mako). Experiments with dielectric walled drift pipes in which electrons escaping laterally from the virtualanode cause ions to be knocked out from the dielectric as it progresses down the pipe have been done by Pasour (NRL), Gullickson (AFOSR), Doggett (NCSU), and Greenwald (Spire). Generally these experiments led to intense fluxes of protons with energies about three times that of the electrons, although somewhat higher energies were achieved at PI with beams approaching the Alfven limit.

Much higher energy protons have been produced in a system developed by Luce (EGG). Here the anode is an insulator with an on-axis hole in it (Figure 5). While the mechanism is not well understood, it appears that a virtual cathode is formed downstream of the anode, electron bombardment causes flash-over of the insulator thus liberating ions that are accelerated to the virtual cathode and may pass through it to form a virtual anode. This in turn attracts the electrons and a bootstrap action persists for some distance. Proton energies up to 60 MeV have been observed. These results have been widely confirmed at Maryland, Cornell, AFWL, Doeing (Seattle). At the last institution Adamski has proposed a scheme in which the Luce diode can be extended to at first two and, later, several stages to obtain still higher energies.

Three schemes are under study in which a potential well is propagated at a programmed and increasing velocity by means of externally controlled elements. Rostoker (UCI) proposes to inject ions from a pulsed wall plasma to speed up the virtual cathode in a time-programmed manner. With the present equipment he hopes to achieve 10 MeV protons and feels it can be extended to 100 MeV. In the Ionization Front Accelerator (IFA), under study by Olson (SA), the electron beam is injected above the limiting current into a tube containing a low-pressure working gas, which has been chosen to be cesium vapor (Figure 6). The pressure is low enough that for the duration of the beam pulse there is not enough ionization to allow the beam current to become less than the limiting current and so to propagate quickly. Arranged along the side of the tube are a series of light-pipes through which carefully timed pulses of laser light can enter to ionize the cesium. Enough ions are produced up-stream of the virtual cathode at the head of the beam to neutralize the beam space-charge in that region and reduce the potential to zero. Downstream of the beam head there is little beam present and the potential is essentially zero there, also. Thus the potential well in the beam head provides an accelerating bucket which, in fact, has quite a sharp gradient at the upstream side. By gradually advancing the region of ion creation by successive laser light pulses the well can be nudged forward at a predetermined rate to accelerate ions. In an initial set of experiments the controlled beam front motion has been demonstrated. By introducing some hydrogen gas and setting the sweep-rate of the beam front, Olson has observed proton acceleration; the data suggest that controlled accelerating fields of 50 MV/m have been achieved.

Because it is unusual in having several features externally controllable the Collective Particle Accelerator (CPA), now under test by Friedman (NRL), is an especially interesting concept. In this, a hollow electron beam is injected below the limiting current, passed through a chopper to create a sequence of rings of charge which then enter a guide field made up of discrete short solenoids. As the train of rings passes down the rippled guide-field their radii throb alternately inward and outward. This produces an axial electric accelerating field which can be decomposed into two waves, a slow forward wave (v is less than c) and a backward wave which can be either slow or fast, depending on the choice of parameters. The phase velocity of the accelerating wave can be controlled by varying either the inter-ring spacing or the inter-magnet spacing, and its amplitude can be varied by changing either the beam current or the magnetic field strength. Note that the mechanism proceeds by the action of discrete rings of charge

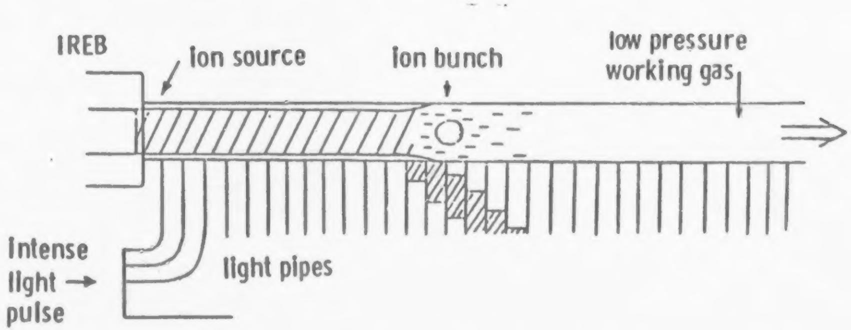

IREB
ion source
ion bunch
low pressure working gas
Intense light → pulse
light pipes

Fig. 6: Schematic of the Ionization Front Accelerator (IFA). The beam behind the beam-front has been charge-neutralized by earlier laser-ionization of the working gas; ahead of the front no significant amount of beam is present. Thus the beam-front forms a deep potential bucket which can be edged forward at a controlled rate by laser-ionization at its rear end.

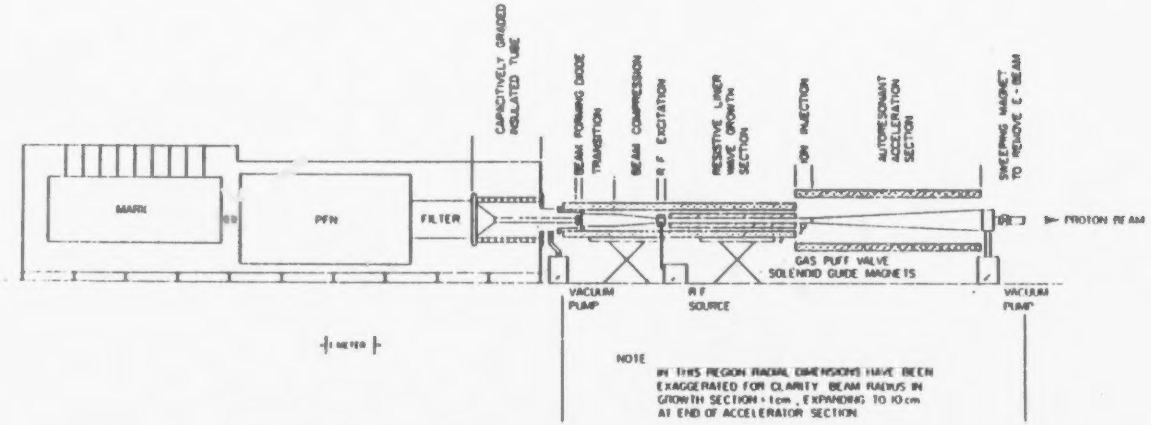

MARX
PFN
FILTER
CAPACITIVELY GRADED INSULATED TUBE
BEAM FORMING DIODE
TRANSITION
BEAM COMPRESSION
R F EXCITATION
RESISTIVE LINER WAVE GROWTH SECTION
ION INJECTION
AUTORESONANT ACCELERATION SECTION
SWEEPING MAGNET TO REMOVE E-BEAM
PROTON BEAM
GAS PUFF VALVE
SOLENOID GUIDE MAGNETS
VACUUM PUMP
R F SOURCE
VACUUM PUMP
1 METER
NOTE
IN THIS REGION RADIAL DIMENSIONS HAVE BEEN EXAGGERATED FOR CLARITY BEAM RADIUS IN GROWTH SECTION = 1cm, EXPANDING TO 10cm AT END OF ACCELERATOR SECTION

Fig. 7: Schematic of the auto-resonant accelerator (ARA).

each of which retains its identity, and thus is quite different from the wave accelerators discussed below. It is clear that the electron rings act also to produce radial focussing of the ions. For experimental convenience, the present tests at NRL are aimed at using the backward wave to demonstrate ion acceleration. In practice, the forward wave would seem to be easier to control.

As of early 1980, experiments have proceeded to the point of generating a train of discrete rings and propagating them along the bumpy solenoid guide-field for a distance of several meters.

3. Acceleration by Waves on Intense Relativistic Electron Beams

Another technique for the controlled collective acceleration of protons uses a negative-energy wave train grown on an electron beam propagating in a vacuum and confined by an axial magnetic field. There are two experiments in progress for the last couple of years to study the potential for application of variable phase-velocity wave-trains to collective acceleration. Both have recently succeeded in demonstrating the excitation and growth of the wanted waves and the suppression of other unwanted modes, but have not yet reached the point of injecting ions. Although each of these experiments utilizes waves of very different character -- one a cyclotron wave, the other a space-charge wave -- both have the feature of exploiting a negative energy mode. Thus, the greater the number of ions accelerated the larger the amplitude of the accelerating field grows (until non-linear saturation occurs). Large-amplitude wave excitation creates a longitudinal modulation of the space-charge potential, and hence a sequence of accelerating buckets, which propagate along the beam with the phase-velocity of the wave. Acceleration of the ions is achieved by arranging for the phase velocity to increase from some initial low value at injection.

(i) Auto resonant accelerator (W. Drummond, ARA, Inc.): This system utilizes a cyclotron wave (so-called lower-hybrid Doppler-shifted cyclotron mode) which has the attractive feature that the phase velocity can be made very small; thus ions can be picked up from rest by simply injecting a puff of gas at the appropriate place. Thereafter the field can be diminished in a tapered way, the phase velocity increased and the ions accelerated.

A proof-of-principle experiment is underway at Austin following extensive theoretical analysis (Figure 7). The procedure is to pass the beam (2.5 MV, 20 kA) through a double-helical resonant excitation section driven at 250 MHz to excite the wave. Next comes a helical growth section which loads the wave and causes it thereby to increase in amplitude. This has now been accomplished (and incipient non-axisymmetric modes suppressed) and potential wells of about 200 kV demonstrated, in modest magnetic fields (2 kG). Since the phase velocity is not small the next step will be to pass the beam into a tapered solenoid (from 2 kG up to 20 kG) to slow down the wave to the point where ion pick-up is possible. Finally the flared-field accelerating section will be added.

(ii) Space-charge wave accelerator (J. Nation, Cornell; P. Sprangle, NRL): The second system being studied for acceleration employs a negative-energy slow space-charge wave grown on the beam during its propagation through a slow-wave excitation structure. The behavior of slow space-charge waves has been long studied in the case of vacuum tubes but there are some differences for relativistic high-current beams.

It was pointed out by Sprangle et al. (NRL) that an accelerator could be built by injecting a beam of constant diameter into a pipe whose walls converged in tapered fashion towards the beam (Convergent Guide Accelerator, or CGA). Under such a circumstance the phase velocity, v_{ph}, can be made to increase gradually as needed. Sprangle et al. gave arguments showing that such an accelerator could give 0.5 amps of protons at 300 MeV in a length of 15 meters.

Upon analysis, however, it turns out that one cannot practically realize low values for v_{ph}, in contrast with the cyclotron wave case. As v_{ph} goes to 0, the beam current I goes to I_L, the limiting current. Values below 0.2 c seem impracticable, thus such an accelerator will require an ion injector.

Experiments by Nation at Cornell in the last two years with a 250 kV beam have shown successful growth in an iris-loaded structure of a slow wave at 1.1 GHz. The growth is rapid (5 db/iris) and accelerating fields of 6 MV/m were generated. In order to study low phase velocities, he used a flared beam pipe to slow down the wave (the opposite of the CGA) and found that below $v_{ph} \approx 0.2$ c, operation was so close to the limiting current that the system was grossly erratic. At this time he is ready to inject protons at about 20 MeV into the wave from a Luce diode or an induction linac in order to study trapping and acceleration. Related studies are also being carried on by Sprangle et al. at NRL, who have considered a periodically loaded wall, to allow lower velocities to be achieved.

IV. SUMMARY AND CONCLUSIONS

Although there is obviously much more work to be done in the areas of accelerator research and development discussed in this Appendix, there has, nonetheless, been very significant progress in the last few years in understanding and developing high-current accelerators and collective effect devices. Among the more important advances one can list the following:

- A new level of sophistication has been introduced into the diagnosis of emittance behavior in conventional proton rf linacs. (AT-2)
- The frontiers of beam current and energy are being advanced for the electron induction linac. For years this has been a high repetition-rate pulse power device in the 1 kA, 5 MeV range: now beam currents are an order of magnitude greater and soon the energy will be increased by a like factor. (ATA)
- In the electron beam current range of 100 kA, the energy frontier is also being advanced by addition of multiply-staged pulse-power accelerating modules such as in the RADLAC and the autoaccelerator.
- Although still far from complete, understanding of neutralization techniques for providing strong transverse focussing for intense beams has advanced considerably (e.g., in the Pulselac and Toroidal Ion Accelerator experiments). Such techniques will undoubtedly provide a

valuable future tool in accelerator technology.

- A new generation of experiments has begun in collective acceleration of ions. The generation of high fluxes of low energy ions by electron beams in gas-cells and related devices is now rather well understood. The accent now is on devices that can be scaled to higher ion energy (1 GeV). These include multiply staged devices such as cascaded Luce diodes, wave accelerators (e.g., ARA, Cornell) and externally-controlled moving potential wells (e.g., IFA, CPA).

The broad goals of most of the accelerator R&D addressed in this Appendix have been two-fold: the advancement of high-current relativistic electron-beam technology and the acceleration of ions from low energy. The latter is an especially difficult problem because of the need to control collective fields that move very slowly initially and can be later speeded up in a programmed fashion; essentially no thought has been given to the different question of collective acceleration of ultra-relativistic particles travelling with almost constant velocity. In fact, how this type of R&D may in future bear on advancing the art of HEP accelerator technology is unclear, partly because it is an unexamined question but largely because by its very nature it is a long-range research field. It is complicated research still in an emergent stage; which effects and devices will pay off in the future, what their unique advantages may be, or what things are still undiscovered, are all unanswered questions.

One can perceive certain areas that could be of interest in the future of HEP accelerators as we today imagine they may be. Injectors that are compact or have some special feature (e.g., short pulse, high-intensity) are an obvious example. The scale of future e^+e^- storage rings is such that pulse-power systems might become of interest to replace the rf systems. Beam extraction and manipulation will pose serious problems in TeV-class machines because of the material limitations of the septa; collective effects could be used to provide strong fields without such material problems. Direct coupling between waves on an electron beam and a relativistic proton beam is conceivable as a way to avoid losses. Pulse power technology could lead to short wavelength sources to test the laser-acceleration mechanisms. Speculation can only be vague and it is clear that much more research is needed before the good ideas and their applications can be put in perspective.

Because of the potential for impact on HEP technology in the future, it is clear that R&D on novel principles of acceleration or related tools should be regarded as a valid candidate for AARD support from HEP. It is important for high-energy physicists to keep informed about developments in this field and that the HEP funding agencies be receptive to proposals for R&D to advance our understanding in this fascinating and complicated branch of accelerator physics.

V. ACKNOWLEDGEMENTS

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APPENDIX II

LASER DRIVEN ACCELERATORS

I. INTRODUCTION

II. LINEAR MECHANISMS

  1. Inverse Cerenkov Radiation
  2. Inverse Transition Radiation
  3. Inverse Synchrotron Radiation
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III. NON-LINEAR MECHANISMS

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APPENDIX II

LASER DRIVEN ACCELERATORS

I. INTRODUCTION

Research with high energy elementary particles has been dominated by circular accelerators, with the one exception of the Stanford Linac. A proton synchrotron built with conventional magnets is limited to an effective gradient (top proton energy divided by circumference) of less than 100 MeV/m. Because of difficulties with forces and materials, it is improbable that high field superconducting magnets will allow effective gradients much above 400 MeV/m. In the case of electron synchrotrons, synchrotron radiation forces a decrease in effective gradient as the peak electron energy rises: for the largest electron synchrotron designed, the effective gradient is about 25 MeV/m or less, depending on the amount of accelerating power installed. The Stanford Linac with the SLED improvement yields about 20 MeV/m. The accelerator structure could support about twice that gradient, if the rf power were available.

Length of the accelerator structure is probably the most important single factor determining costs, though the nature of the equipment and the levels of peak and average power consumption are obviously very important. We note that at LEP energies we have approached the point where the effective gradient crosses over that provided by a fairly conventional linac. Electron beams of still higher energy are almost surely provided at lower cost by linear accelerators. In the case of proton and heavier ion accelerators, it is also true that substantial increases in the effective gradient are needed if we are to maintain our pace of increase in maximum energy at roughly fixed costs. However, the goal for an attractiwe linear accelerator in this case must be of the order of 1 GeV/m, far beyond current practice.

One parameter of such a high gradient linear accelerator is worth pointing out. If a bunch of 10^{11} particles is accelerated by 1 GeV/m, energy is being put into the beam at a rate of 5 x 10 watts. The power fed into the accelerator will be greater because of finite efficiency. It is clear that the power sources must be different from those used in conventional linacs. One possibility is new types of power sources based on radiation by particle beams in material or magnetic structures: the evolution of conventional rf tube development (or devices based on rf storage) is mentioned in the text and described in Appendix III. Another power source which has a demonstrated capability to provide the power levels needed is lasers based on atomic systems. The power source for the acceleration mechanisms described below could be such a laser, or a free electron laser or other power source of the first class. The distinguishing element is that the wavelength must be short enough so that optical techniques may be used to concentrate the rf to high power densities.

II. LINEAR MECHANISMS

Many schemes have been described for coupling the laser power to the particle beam. Unfortunately an electromagnetic plane wave packet acting on a free particle does not result in a net acceleration, and thus it is necessary to introduce some other bound or free charges near the particle beam. The best understood mechanisms are those that utilize the electric field in a slow wave structure excited by the laser. Their operation can be easily understood by referring to the inverse process, radiation generated by a particle passing through the structure.

1. Inverse Cerenkov Radiation

When a particle traverses a medium it will radiate at a given angle depending on its velocity and the index of refraction of the medium. Then if a light beam of the correct polarization were to propagate in the sense of motion of the particle and at the radiation angle, it will clearly have the correct phase relation to put energy into the motion of the particles. If the particle lies at the correct phase of the oscillating wave, it will receive a continuous acceleration. That phase relation must clearly remain correct over the length of the accelerator. This is a severe technical challenge for a high energy accelerator whose length must be of the order of a kilometer if the wavelength is in the visible or infra-red range. The medium could be a low atomic number gas, in which case the structure is relatively simple. This mechanism has been demonstrated in an experiment which achieved an accelerating gradient of 0.2 MeV/meter.(1) This figure has since been somewhat increased. If the emittance growth due to passage through the gas is unacceptable, a solid material with a hole of diameter of the order of the wavelength of the light could be used. The effective gradient is limited by breakdown in the refraction medium, and is probably higher in the gas, perhaps approaching 1 GeV/m. If longitudinal stability is necessary, adequate quadrupole focussing must be provided, to maintain an acceptance which is at best rather small.

2. Inverse Transition Radiation

A particle moving through a periodic structure generates transition radiation. Smith and Purcell,(2) for example, observed light radiated by electrons passing near a plane grating. The inverse effect may be used to accelerate a particle passing near a grating illuminated by radiation at the right angle and polarization.(3) The structure must be fabricated with an accuracy of a fraction of a wavelength, but the particle beam need not pass through any material, as long as it is positioned in the near field of the grating. The advantage of this structure over the previous one is the absence of multiple scattering, perhaps more feasible methods of manufacture and alignment, and the probable higher effective gradients allowed by breakdown on reflecting surfaces rather than refracting media. Also, a damaged point on a grating can be replaced by a small motion along the grating lines. This device is a close relative of an ordinary rf linac, with an unusual cavity structure and rf feed mechanism.

3. Inverse Synchrotron Radiation

A static magnetic or electric field transverse to the particle direction can be used to couple a particle beam to the radiation field, as in the wigglers used in electron storage rings. They can be used to keep a particle in phase with a light beam incident along the particle beam axis and thus achieve acceleration.(4) Unfortunately, the magnetic field needed to obtain efficient coupling becomes very large as the particles become relativistic. The method might find useful application in some circumstances.

4. Inverse Cyclotron Radiation

In this case the magnetic field is directed along the particle direction and the phase matching is due to the cyclotron motion of the particles.(5) The magnetic field requirements for relativistic particles become still more difficult to satisfy.

III. NON-LINEAR MECHANISMS

There is another class of mechanisms which are driven by the power density in the rf field acting on a plasma. Since these involve the square of the electric field strength, they are known in plasma physics terminology as non-linear interactions. The attainable gradients are limited not by breakdown, but by the power available, and possible certain plasma instabilities. Another advantage is that the scale of dimensional accuracy required is set by the size of the focal spot rather than by the rf wavelength, and phase coherence is not necessary. On the other hand, very intense laser beams are needed to excite these interactions efficiently, and the analysis of an acceleration mechanism is not nearly as straightforward as for the linear mechanisms.

A simplified picture which gives a physical insight into the non-linear mechanism is shown in Figure 1. This shows a series of electrons uniformly spaced along a line, with a short wave packet of radiation incident from the left. The combined action of the electric and magnetic field on the electron produces a forward "hopping" motion of the electron, which returns to rest once during each cycle of the passing electromagnetic wave.(6) After the wave packet is past, the electrons are once again at rest, but displaced along the axis. It is seen from the Figure that this phenomenon produces an enhanced concentration of electrons in the region of the wave packet. The collective field of this region of enhanced charge density can be used to accelerate and focus a bunch of beam particles. In a sufficiently dense plasma, it will excite waves of electron density and ion density.

Figure 1 depicts the situation in which the electron density is sufficiently low that the collective field does not appreciably affect the electron motion. As the density is raised, the field at the edges of the wave packet will oppose the hopping motion, so that the density enhancement is determined by the force which the electromagnetic wave can provide. This is called the ponderomotive force.(7)

The static field at the trailing edge of the wave packet will have the correct sign to accelerate positive particles, and over a certain region will have the correct shape to provide longitudinal stability.(8) Since the charge density is concentrated on the axis of a focal spot, there will also be strong radial focussing. The free electrons can be provided by a normal plasma of low atomic number, since the hopping motion is negligible for the positive ions, in the inverse ratio of their masses. For a relativistic particle beam, it is easy to adjust the group velocity of the wave packet to the velocity of the particles. The density must be high enough to provide sufficient charge to reach the longitudinal field available for a given laser power, but is limited by the necessary group velocity. Higher ultimate effective gradients are available at shorter wavelength. In the infra-red region, computer simulations have shown that gradients of hundreds of GeV/m are in principle possible.(9) Such linear accelerators could indeed be competitive with proton synchrotrons.

The simple picture of Figure 1 does not take into account plasma oscillations. The most prominent effect under the relevant conditions is inverse Raman scattering (9), generating waves of electron energy in the wake of the leading edge of a long wave packet. The static field due to these waves can be used for accelerating particles in the same manner as the single bunch due to a very short wave packet. The sharp leading edge, at least, is a necessary requirement for an efficient acceleration with this mechanism. The laser must have not only very high peak power, but a large bandwidth, to allow a sharp rise and preferably a very short pulse. Efficient lasers with these properties would have to be developed for this purpose, to make it possible to build a useful accelerator.

The general characteristics of the ponderomotive force have been verified by a number of experiments and energetic particles accelerated by the static collective fields generated in this way have been observed emerging from laser bombarded plasmas.(10) An experiment involving acceleration in a controlled, moving electron bunch has not yet been performed.

IV. THERMO-ELECTRIC MECHANISM

If an intense light beam is absorbed at a solid-vacuum interface, at intensities below those where the non-linear interactions dominate, the principal mechanism is inverse bremsstrahlung. Unlike all the other mechanisms which have been described above, the energy goes into a thermal motion, primarily of the electrons. These electrons stream out of the dense plasma at the surface and into the vacuum, followed by the slowly moving plasma front. A transient, strong electrical field is thereby generated. It is easy to see how this could be used for generating very strong focussing fields, and it may be of interest for accelerator configurations as well.

V. SUMMARY

This brief description of accelerators driven by lasers or using optical focussing techniques illustrates the fact that the proposed devices span a wide range of physical effects, and the kinds of power source and necessary research are very different. Better theoretical understanding of the problems involved and the systems applications would seem the logical first steps toward reaching the goals stated in the introduction.

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APPENDIX III

NEW RF POWER SOURCES

The radiofrequency power sources now used in linear accelerators and synchrotrons, triodes and klystrons, have been brought to a high state of development after many years of effort. In the last few years new devices have begun to attract attention by virtue of their capabilities for providing higher efficiency, higher peak power, or by extending the frequency range in which very high power is available. There are still newer devices which have not been demonstrated in operation, but which appear promising on the basis of calculations.

1. The Gyrocon

A new microwave power source using deflection of an electron beam in a magnetic field, has attracted attention. It is mentioned in the report on work in the Soviet Union. The claim of high efficiency has been verified, and devices have been built to deliver very high power.(1) Work on related devices is going on in this country.

2. The Gyrotron

The gyrotron was also first developed as a high power device in the Soviet Union,(2) although the basic principle was demonstrated here.(3) After a long and serious effort gave strikingly successful results, the development was taken up also in the U.S.(4) This device is based on cyclotron radiation by an electron beam in a magnetic field. It is particularly effective in the millimeter region, where it has provided average powers many orders of magnitude higher than previous devices. The recent developments are intended for application in the fusion program, and are concentrated on long pulse devices. Development aimed at short pulses and very high peak powers might be interesting for accelerator applications.

3. The Megatron

A klystron based on multiple arrays of electrostatic quadrupole focused electron beams has recently been suggested. It has not yet been developed in practice, but it may offer another possibility of high peak powers at short wavelengths, by virtue of its small and bright electron beams, while maintaining large total currents through a great multiplicity of beams.(5)

4. The Free Electron Laser or FEL

For which studies exist with great variations of parameters, is interesting for accelerator applications as a very high power, short pulse device with reasonably high efficiency. The version using a very high current, intense relativistic electron beam or IREB, to generate millimeter waves has already been demonstrated.(6) The FEL for shorter wavelengths has also been demonstrated at low power.(7) The FEL for high power at shorter wavelength depends on high current linear accelerator beams, and is still at the stage of design studies. Again, none of these studies has been aimed at accelerator applications.

5. Other IREB Devices

Other IREB devices have been demonstrated, including a version of a cyclotron radiation device,(8) and the Dopplertron in which an rf pulse if backscattered from an IREB in a region of rapidly falling charge density, both gaining energy and being compressed in time by a Doppler shift.(9) This shifts the frequency upward, and greatly increases the peak power.

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APPENDIX IV

ACCELERATOR R&D ON EXISTING MACHINES

Questionnaries were sent to laboratories with major HEP machines. For each machine, information was requested separately for accelerator R&D to improve the present machine performance and for R&D aimed at mid- and long-range goals. The responses are summarized in the following tables.

ACCELERATOR R&D TO IMPROVE PRESENT MACHINE PERFORMANCE - U.S.

Machine	Fraction of Operating Time Devoted to This R&D	R&D Topics in Past 2 or 3 Years	Future R&D Topics
FNAL Linac and Booster	3%	Linac controls gas barriers sparkgap protection modulator improvements Booster Yt jump 8 -> 10 GeV extraction superdamper H^- source	New "rfq" to replace first 2 MeV
FNAL Main Ring and Switchyard	7%	2-turn injection Power supply chokes Abort Magnet improvement Extraction instrumentation Meson 3-way split Septa	New extraction improvements Better Septa
AGS	5%	New slow extraction H^- injection New fast extraction Yt jump	Better rf for linac Polarized protons Better control system
SLAC Linac	3%	Electron gun optics Increase positron yield nsec pulses from guns SLED Beam position monitors Beam breakup Polarized electron sources	Instrumentation and control for SPC
SPEAR	15%	Beam-beam limit Flip-flop instability Synchrobetatron resonances Bunch-lengthening cavity	Wiggler magnets to increase luminosity
CESR	25-35%	Diagnostics Beam gymnastics Single beam instabilities Intensity from linac and synchrotron	Beam instabilities, both single and beam-beam
ZGS	5%	Polarized deuteron acceleration Crossing of imperfection resonances Polarized proton ion source development	

ACCELERATOR R&D ON PRESENT MACHINES FOR LONG-RANGE GOALS - U.S.

Machine	Fraction of Operating Time Devoted to This R&D	R&D Topics in Past 2 or 3 Years	Future R&D Topics
FNAL Main Ring and Switchyard	6%	Beam loss on SC magnets Saver injection DO extraction (new scheme) Power supply ripple	Simultaneous operation of Main Ring and Saver Quenching with beam Beam storage Injection, abort, extraction
AGS	1%	Feasibility of polarized protons in AGS	Better linac rf technology Non-destructive phase space monitor
SLAC LINAC	5% in 1980 (little until recently)	Single bunch emittance measurements Beam breakup measurements Accelerator rf steering measurements Single-bunch position monitor tests	Laser gun emittance measurements Positron target yields Single-bunch diagnostics and control New accelerator structure tests
SPEAR	5%	Beam-beam limit with little damping Bunch lengthening over wide range of conditions	Continuation of current work
ZGS	1-2%	H^- polarized ion source Polarized proton storage studies	

ACCELERATOR R&D TO IMPROVE PRESENT MACHINE PERFORMANCE - NON-U.S.

Machine	Fraction of Operating Time Devoted to This R&D	R&D Topics in Past 2 or 3 Years	Future R&D Topics
CERN PS (Linac, Booster)	6%	Working points (Qh, Qv), stop bands Instabilities Diagnostics	Beam behavior with modified densities (longitudinal and transverse) Fast accelerating cycles
CERN SPS	5%	Resonant extraction Multi-batch injection Synchrotron light from protons Increase of intensity	Increase of intensity $\bar{p}p$ collider tune up Five batches from injector Extraction at higher intensity
CERN ISR	15%	Stochastic cooling $\bar{p}$'s in ISR Beam characteristics via Schottky noise Overlap knockout Microwave instabilities	Superconducting low beta $\bar{p}$ operation
DESY Synchrotron	3%	Injection orbit Feedback for head-tail instability	Beam diagnostics Orbit correction Electronics
DORIS	5%	Beam-beam int. Instabilities Satellite resonances Energy >5 GeV	Mini-beta sections Wiggler
PETRA (includes "long term" R&D)	30%	Space charge limit on luminosity Satellite resonance Automatic tune regulation Vertical instabilities	Second harmonic for bunch lengthening Cavity with higher shunt impedance Mini-beta insertions

ACCELERATOR R&D TO IMPROVE PRESENT MACHINE PERFORMANCE - NON-U.S. (cont.)

Machine	Fraction of Operating Time Devoted to This R&D	R&D Topics in Past 2 or 3 Years	Future R&D Topics
ADONE (includes "long term" R&D)	4%	Wigglers Backscattered laser Orbits Operating points Design studies	Free electron laser experiment Control system Injector New rf system to improve longitudinal stability
Saturne	10%	Beam performance using stored parameters Longitudinal instability Double extraction Working point Heavy ion sources (CRYEBIS) Low-intensity tuning	Stability Flexibility Extraction at two energies New injector (MIMAS)
KEK Synchrotron	20%	Beam intensity Head-tail instability Slow extraction Magnetic property of beam pipe Reliability	Polarized beam acceleration Utilization of booster beam H^- injection

ACCELERATOR R&D ON PRESENT MACHINES FOR LONG-RANGE GOALS - NON-U.S.

Machine	Fraction of Operating Time Devoted to This R&D	R&D Topics in Past 2 or 3 Years	Future R&D Topics
CERN PS	4%	Injection and acceleration of vertically stacked booster beams Longitudinal combination at high energy of intense bunches Deceleration of protons for electron and stochastic cooling	$\bar{p}$ acceleration for SPS $\bar{p}p$ $\bar{p}$ deceleration for LEAR Stochastic extraction and stochastic debunching for LEAR
CERN SPS	10%	Bunch lifetime Emittance growth New beam monitors	$\bar{p}p$ tune-up pp improvement of performance
CERN ISR	5%	SC quads for low beta insertions Cold bore Beam-beam interaction Bunched beam lifetime	Beam dynamics Beam-beam effects $\bar{p}p$ tune-up
DESY Synchrotron	3%	High-density single bunch acceleration	Change in optics for horizontal damping Increase injection energy Modify ejection channel to improve optics
DORIS	3%		SC rf cavity
KEK	3%	SC magnet SC rf cavity Ultra-high vacuum	SC magnet SC rf cavity Colliding beam machine physics

THE U. S. ENERGY RESEARCH & DEVELOPMENT ADMINISTRATION REPORT OF THE HIGH ENERGY PHYSICS ADVISORY PANEL 1977 SUBPANEL ON NEW FACILITIES

JUNE 1977

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

July 6, 1977

Dr. James S. Kane
Deputy Assistant Administrator
for Physical Research
Energy Research and Development Administration
Washington, D. C. 20545

Dear Jim:

I am forwarding to you the report of the 1977 HEPAP Subpanel on New Facilities which was formed in response to the charge given to it by you at the HEPAP meeting of February 18-19, 1977 in Washington, D. C.

This report was discussed by HEPAP at its June 27-28 meeting in Washington; and the recommendations contained therein were unanimously endorsed. We urge you to take all possible steps to effect their realization.

The discoveries made in the last two years and the technical advances in the accelerator art have reenforced our view that the fundamental strategy of the approach to higher energy via development of electron-positron and proton-proton colliding beams and high energy fixed target facilities is essential to successful investigation of the fundamental structure of matter. ISABELLE is now the critical feature in this program and its construction should begin as soon as possible.

The ISABELLE proton-proton colliding beam facility provides an increase of a factor of more than ten in the center-of-mass energy over that available in the highest energy collisions that are now possible at Fermilab and at CERN.

With this advance of the high energy frontier we expect, on the basis of present theoretical ideas, to cross the threshold for producing the massive fundamental particles that are believed to be the quanta, or carriers, of the weak forces of β radioactivity. The observation of these hypothetical particles will be a major triumph for our current concepts; and the opportunity to explore their properties will be of fundamental importance. On the other hand, failure to confirm their existence in experiments at the very high energies of ISABELLE would also have a very major impact on our understanding of elementary particle interactions. The actual energy recommended by the Subpanel and endorsed by HEPAP, namely a maximum of about 400 GeV for each beam, is higher by a factor of two than that originally proposed for the ISABELLE project. This higher energy is justified by the greatly enhanced physics potential of the facility even though an increase in cost is required. The high luminosity expected coupled with the flexible design of the interaction regions will make possible a broad and rich program of experimentation at ultra high energy. As already emphasized in previous Facilities Subpanel reports, such a facility must be a major part of our future thrust in High Energy Physics.

Since the report of the 1975 Subpanel on New Facilities was issued there have been great strides in high energy physics, highlighted by the discovery of particles exhibiting the new quantum number "charm." We are very appreciative of the considerable efforts by ERDA and the Congress which reversed previously decreasing budgets and have provided a measure of increased support to the field of high energy research. We hope that this trend will continue in the future so that, within the given budgetary guidelines, we can maintain a balanced program utilizing existing facilities and taking advantage of the technical and scientific opportunities for new exploration. With the positron-electron project (PEP) now under construction we have made a start on the three-pronged program which is essential for exploring the fundamental structure of matter. Construction of ISABELLE is now the critical next step to be taken in implementing this strategy.

With best personal wishes.

Sincerely yours,

Sidney D. Drell

Sidney D. Drell
Chairman, High Energy Physics
Advisory Panel

sj

Yale University New Haven, Connecticut 06520

PHYSICS DEPARTMENT
217 Prospect Street

June 24, 1977

Dr. Sidney Drell
Chairman
High Energy Physics Advisory Panel
c/o Stanford Linear Accelerator Center
P.O. Box 4349
Stanford, California 94305

Dear Sid:

I am writing to transmit the report of the 1977 HEPAP Subpanel on New Facilities which met in Wood's Hole from June 5 to June 11, 1977.

There is little that I can add, except to say that the subpanel was unanimous in its recommendations.

With best regards.

Sincerely,

Jack

Jack Sandweiss

JS/jlf

SECTION I

INTRODUCTION

High Energy Physics Advisory Panel (HEPAP) Subpanels appointed in 1974 (the Weisskopf Subpanel) and 1975 (the Low Subpanel) have considered and made recommendations on the construction of those future facilities required to maintain the vitality of the field of High Energy Physics in the United States. The recommendations of the 1975 Subpanel, in brief, were:

" I. ...authorization in FY 1976 of the LBL/SLAC proposal to construct the electron-positron colliding beam facility PEP...

" II. ...authorization in FY 1977 of the BNL proposal for the construction of the proton-proton colliding beam facility ISABELLE. This facility is designed to operate at energies between 30 and 200 GeV per beam with a peak luminosity of $10^{33}cm^{-2}sec^{-1}$...

"III. ...that funds be provided to support an accelerator development program at Fermilab directed toward the long-term goal of fixed target and/or colliding beam systems in the region of 1000 GeV and above, and we recognize the Energy Doubler/Saver as a proper step to this end..."

Since the issuance of the report of the 1975 Subpanel, the following important developments have occurred:

- The Stanford Linear Accelerator Center-Lawrence Berkeley Laboratory (SLAC-LBL) Positron-Electron Storage Ring project (PEP), recommended as the top priority construction project by both the 1974 and 1975 Subpanels, has been authorized and is now in the process of construction.
- Although the Brookhaven National Laboratory (BNL) proton-proton storage ring ISABELLE has not yet been authorized, there has been very substantial progress in the research and development (R&D) effort, and serious consideration has been given by the BNL group to the design of a higher energy version of the machine.

- The Fermi National Accelerator Laboratory (Fermilab) Energy Doubler/Saver (ED/S) R&D project is well underway and very substantial advances in development of ED/S superconducting magnets have been achieved. Furthermore, the investigation of techniques which offer the possibility of useful luminosity in proton-antiproton colliding beams has been initiated.

- The pace of new discoveries in High Energy Physics has continued unabated. Conclusive experimental proof for the existence of a new property of matter entitled "charm" has been obtained, and evidence for other remarkable phenomena has been uncovered.

Thus, the program recommended by the 1975 Subpanel is underway, although at a slower pace than envisaged.

The results of the R&D efforts of the last 2 years have been incorporated into several new or updated construction proposals.

The 1977 Subpanel on New Facilities was formed in February 1977. The Charge to the Subpanel is given in Appendix A.

The specific construction projects proposed by the laboratories for FY 1979 funding were:

1. ISABELLE (BNL)

This project consists of a proton-proton colliding beam facility (ISABELLE), employing superconducting magnets, to contain beams injected from the BNL Alternating Gradient Synchrotron (AGS) and accelerated to full energy within the ISABELLE rings. The initial proposal was for beams of energy up to 200 GeV per beam and estimated luminosities up to $10^{33}\text{cm}^{-2}\text{sec}^{-1}$. An alternative proposal, of great physics interest, made possible by recent improvements and economies in magnet performance and accelerator design, describes a project that can achieve energies up to about 400 GeV per beam.

2a. Energy Doubler/Saver - Tevatron (Fermilab)

The Tevatron is based on the present R&D project to develop a coasting beam in a ring of superconducting magnets to be installed in the Fermilab main ring tunnel. The formal construction proposal from Fermilab comprises:

i) conversion of the ring of superconducting magnets to an accelerator capable of attaining proton energies of 1000 GeV

(1 TeV), with an appropriate extraction system, and an average intensity of at least 10^{12} protons per second;

ii-iv) modifications of the Neutrino, Proton, and Meson Areas for use with 1000 GeV protons.

2b. Colliding Beams Experimental Facility (Fermilab)

The development of a capability to bring counter-rotating proton beams in the Doubler/Saver and main ring into collision is proposed. A further aspect of this effort is the production of collisions between counter-rotating proton and antiproton beams in the Doubler/Saver ring. A formal proposal to implement such use comprises:

i) the provision of detector enclosures at each of two intersection points, together with major technical components and associated service facilities;

ii) an antiproton production facility consisting of a 100 GeV proton extraction system in the main ring, a beam transport system, and an antiproton production target facility.

3. PEP Upgrade (SLAC-LBL)

Included as part of the long range plans of SLAC and LBL was a plan to increase the energy of the PEP electron-positron facility from 15 x 15 GeV to 24 x 24 GeV. This upgrading, whose purpose is to extend the experimental capability into an energy range of particular significance for the study of weak interactions, requires a substantial expansion of the RF system and some expansion of power supplies for the magnets. A formal construction proposal is expected in the near future, but was not available at the time of the Woods Hole meeting.

A preliminary meeting was held at Fermilab on March 17, 1977, to plan the schedule for the work of the Subpanel. The Accelerator Subgroup of the Subpanel, with its consultants, gave specific attention to the technical aspects of the new facilities to be considered by the Subpanel as a whole. The Subgroup made 2-day visits to BNL and Fermilab, respectively, to hear presentations concerning current R&D activities and the construction proposals prepared by each of these laboratories. These visits offered opportunities for technical discussions and the formulation of questions for response prior to the Subpanel meeting.

The full Subpanel met for the week of June 5-11, 1977, at Woods Hole, Massachusetts. The participants in that meeting are listed in Appendix B. In the early part of the June 5-11 meeting presentations and reviews were heard from BNL, Fermilab, SLAC, LBL, and Cornell University followed by a review of the developments underway or in the planning stage in Western Europe. Laboratories had submitted documents detailing their long range plans, and laboratory representatives included discussions of these plans in their presentations. Following these presentations, there was further opportunity for specific questions and discussions with various experts from the laboratories who were present during the second and third days at Woods Hole.

Presentations on the present budget status and on the budgetary context of the Subpanel's considerations were made by representatives of ERDA and the National Science Foundation (NSF).

The deliberations of the Subpanel, although most specifically addressed to the formulation of recommendations relating to FY 1979 funding, involved extensive consideration of the High Energy Physics program for the period through FY 1987 as might best be formulated within the funding constraints given to the Subpanel. Although the laboratory proposals constituted the major basis for the Subpanel deliberations, the Subpanel felt free to consider variations of these proposals as well as additional possibilities, and also considered ways of providing opportunities for the study of and, near the end of the decade, for a start on the construction of advanced new facilities that might be required in the subsequent decade.

The Subpanel interpreted the budget guidelines given to it as constituting annual costs in FY 1978 dollars averaged over the 10-year period 1978-1987.

The Subpanel recommendations are consistent with the 10-year goals as expressed by both the 1974 and 1975 Subpanels--exploration of the frontier via a set of fundamentally different facilities. The Subpanel's recommendations would allow research to be carried out with three kinds of complementary facilities: electron-positron colliding beams; proton-proton colliding beams; and high energy beams colliding with fixed targets, while maintaining options for future possibilities as changes in physics and technology may dictate. The Subpanel believes that the steps it has recommended are appropriate ones for FY 1979 in the light of those goals. It is very aware of the fact that both technology and science can change rapidly and that frequent reassessment may be necessary.

The specific recommendations are given in Section IV. The impact of the alternative funding constraints is discussed in Section VI.

SECTION II

NEW DEVELOPMENTS IN PARTICLE PHYSICS

In the past 2 years, we have witnessed not only a broad range of new discoveries, but also growing convictions about their interpretation. By 1975 there existed, as a consequence of the discovery of the J/ψ particle at BNL and SLAC, widespread belief in the existence of a new form of matter--"charmed" matter. Experiments at SLAC, Fermilab, BNL, CERN, and DESY have quite decisively confirmed this expectation. Charmed particles have been studied in considerable detail at the SPEAR and DORIS storage rings, and have been detected at BNL, CERN, and Fermilab in bubble chambers exposed to neutrino beams as well as in counter experiments.

Not only is the charm concept well established, but it also has strengthened the hypothesis that strongly interacting matter (such as protons and neutrons) are composed of subunits called quarks. In quark language, the charm attribute is directly connected with the existence of one more such subunit, the charmed quark, than was previously supposed. The discovery in the electron-positron storage ring experiments of a new family of particles closely related to the J/ψ has lent support to the idea that the J/ψ is a composite of the charmed quark and its antiparticle.

Other indications of the presence of subunits in protons have emerged more strongly in the last 2 years. Evidence for proton subunits in collisions of electrons with protons was found at SLAC several years ago, and now there is growing evidence that when two proton beams collide there also occur violent collisions of subunits. These processes, while more complex to analyze, hold promise of a simple description of an underlying collision mechanism which is dependent on the properties of the subunits at an extremely small distance scale. Thus, these observations hold out the promise of a fruitful and fundamental field of study for high energy fixed target experimentation and especially for higher energy proton-proton and proton-antiproton storage rings. The synthesis of weak and electromagnetic interactions is another concept in which there is growing confidence. This concept was already discussed in the 1974 Subpanel report. Since that time, measurements of neutrino-induced processes have led to quantitative agreement with the theoretical ideas--the so-called gauge theories--based on that concept. The keystone of those ideas is that weak interactions have carriers analogous to the photon: a massive charged particle, the W, and a neutral companion, the Z.

The discovery of the W particle has been a long-standing dream for high energy physicists. At present, the realization of that dream seems closer than ever before. The gauge theories have provided a solid base for anticipation of the properties of the W. Furthermore, as a result of Fermilab and CERN-ISR experiments on the closely related process of production of particle-antiparticle pairs of muons or electrons, a more reliable estimate of the expected rate of production of the W and the Z in very high energy collisions can now be made. Consequently, there is considerable confidence that, if the gauge theories are correct, the next generation of proton-proton storage rings will indeed suffice to find them.

However, it cannot be said that there is now any kind of coherent picture of the constituents of matter. There exists quite a large number of the most basic building blocks, and new candidates for building blocks have already emerged. At SPEAR and its sister machine DORIS in Germany, considerable evidence has been accumulated which is consistent with the existence of a new member of the lepton family. At Fermilab there have also been indications of new leptons and/or quarks produced by neutrinos. No one knows how many more building blocks are needed to complete the picture—or indeed whether that is even the right way of looking at the problem. But if it is, the physicist may now be in a position similar to the chemist discovering the elements, but having in hand only a small fraction of the total. Particle physics may need to find its periodic table, and more of the entries may have to be discovered in order to discern the pattern that leads to further synthesis and economy of description.

The hypotheses of the present need to be tested by the experiments of the future. The Tevatron, PEP, and ISABELLE all open up new prospects for discovering more of the building blocks of matter. The proton-proton colliding beam facility, ISABELLE, and the Fermilab colliding beam schemes allow the exploration of the natural energy domain suggested by the weak-electromagnetic gauge theories. If the W and the Z were to be found with the expected properties, present attitudes regarding the weak-electromagnetic synthesis would become convictions, much as the attitudes in 1975 about charm have now become convictions. On the other hand, were the W and the Z not to be found, there would begin a new revolution in our thinking, as well as new opportunities for direct study of the weak force. Thus, the strongest conviction is that, irrespective of how many new forms of matter are found, the next generation of facilities will provide high energy physics with a major new step in our understanding of the subnuclear world.

SECTION III

TECHNICAL CONSIDERATIONS

ISABELLE is a proposed proton-proton colliding beam facility which constitutes the next natural step beyond the only other existing device of its kind, the ISR at CERN. Protons from the existing 30 GeV accelerator (AGS) at BNL would be injected and stacked in ISABELLE and then accelerated to maximum energy. Possible future additions might include the capability to collide protons with other particles, in particular electrons or antiprotons.

Both the 1974 Subpanel and the 1975 Subpanel considered a version of ISABELLE in which the maximum proton energy was approximately 200 GeV. As has been stated in the reports of these Subpanels, the ISABELLE proposal is a result of several years of detailed study and development and has had the advantage of the experience gained at the ISR. This study, which has actively continued since the previous reports, covers all pertinent aspects of accelerator physics, and strengthens the conclusions of the 1975 Subpanel, that there is a sound basis for proceeding into the construction phase.

The very encouraging results in the development of superconducting magnets suitable for use in ISABELLE and the intense interest of the high energy physics community in carrying out research using higher energy colliding proton beams has led BNL to consider both 400 GeV and 200 GeV versions of ISABELLE. Separate proposals for these two options have been prepared by BNL and presented to ERDA and the Subpanel. BNL is prepared to construct whichever of these options can be best justified in the light of scientific and economic considerations. The 400 GeV proposal expands on the 200 GeV proposal by the use of a longer accelerator tunnel, a somewhat weaker focussing magnetic lattice which allows for more and slightly longer bending magnets within the accelerator, and by operation of the superconducting bending magnets at a peak magnetic field of 5 Tesla rather than the 4 Tesla proposed for the 200 GeV option. The estimated cost of the 400 GeV option would be about 1.4 times the cost of the 200 GeV option. In both cases the estimated maximum luminosity of $10^{33}\text{cm}^{-2}\text{sec}^{-1}$ is above that so far achieved in the ISR by a factor of 20. Convincing evidence is given that this luminosity can be obtained after a period of running-in. Both proposed ISABELLE variants have been designed for a total of six interaction regions; however, in both cases the design could be modified to incorporate eight interaction regions with equivalent machine performance.

Prototype magnets suitable for use in the 200 GeV version of ISABELLE have been constructed and have demonstrated reliable operation at magnetic field levels which are substantially higher than required to achieve 200 GeV and magnetic field quality which satisfies ISABELLE requirements. This performance may ultimately permit the lower energy ISABELLE version to operate at energies appreciably in excess of 200 GeV. Efforts to ensure the ability of industry to build these magnets have begun. A prototype quadrupole magnet has been developed which exhibits performance in excess of requirements. A half-cell consisting of two prototype dipole magnets and one quadrupole magnet has been assembled and operated under computer control. Although this half-cell test is not yet completed, the test system has achieved magnetic field levels in excess of those required for the 200 GeV operation. The Subpanel feels that the proposed cryogenic system will work but believes that further refinements and improvements can and should be made.

A full scale prototype magnet such as would be required for the 400 GeV option has not yet been constructed; however, the required physical construction of the 400 GeV magnet is identical in all respects to that of the 200 GeV magnets except for a 13 percent increase in length. No difficulty is anticipated in accomplishing this relatively minor change. To reach 400 GeV, these modified magnets must operate at 5 Tesla. Fields within a few percent of the required 5 Tesla have been achieved and we believe that the performance goal is reasonable.

An early operational test of a segment of magnet lattice consisting of about 10 magnet units including vacuum, power supplies, computer control, and final cryogenic systems would be a valuable verification of the overall engineering design of the accelerator system.

The Subpanel believes that the technical level of achievement of the ISABELLE research and development program is such that the project is ready for funding either as a 200 x 200 GeV facility or as a 400 x 400 GeV facility.

There is currently an Energy Doubler/Saver R&D project at Fermilab which has the goal of achieving a coasting beam in a ring of superconducting magnets. The recommendation to proceed with this program was made by the 1975 Subpanel.

After the successful conclusion of this R&D program, there will be a number of different ways in which the development can be applied to the physics goals of the laboratory. Among these are operation in the energy saving mode, TeV fixed target mode, and colliding beam modes.

Two construction proposals have been submitted by the Fermilab. One of these is the Energy Doubler/Saver (Tevatron) and the upgrading of experimental areas to exploit the higher energies. The other is the Colliding Beams Experimental Facility.

Because the success of the Energy Doubler/Saver R&D program is essential to the proposed applications, a careful evaluation has been made of its present status and the likely performance levels which will be achieved.

Extensive progress has been made in the last 2 years. At the present time a large number of full length developmental magnets have been produced which operate at a field strength exceeding the design goal of 4.25 Tesla. A string of four such magnets has been operated successfully.

A pilot production line has been set up in the laboratory which at present has the capability of producing several dipole magnets per week. The tooling for this production line, though well advanced, has yet to be refined sufficiently for the final production fabrication. The laboratory believes that further refinement of the design of the structural elements of the magnets and higher precision fabrication tooling will yield magnets of satisfactory quality and reproducibility. The first quadrupole magnet unit has just been completed and is now under test.

The cryogenic and refrigeration system is highly developed and sophisticated and work in this area appears to be proceeding satisfactorily. A satisfactory procedure has been developed for rapid installation of magnets in the ring. Overall, the R&D program is making rapid progress, and the large scale production and assembly tests are most encouraging. The major unknown is the field quality which will be achieved in the final assembly. The various modes of application have different requirements on field quality, and since the ultimately attainable field quality is not yet known, the probability of successful operation cannot be predicted with equal reliability for each anticipated mode.

The Energy Doubler/Saver (Tevatron) proposal requests construction funds to provide for the rapid acceleration of the beam in the superconducting ring to the full design energy, 1000 GeV, and to provide for its extraction to the transport channel. It is expected that the facility will function satisfactorily as planned with single turn injection and fast extraction in the energy doubler and saver modes. The process of slow extraction of the beam during a long flat top is more difficult because of the higher requirements on the field quality and control of the beam. However, we believe this can be accomplished. Great care must be exercised throughout injection, acceleration, extraction, and in circumstances of emergency beam abort to avoid excessive heating which would cause quenching of the superconducting ring and transport elements. The most demanding application of all is the stacking of as many as 10 pulses from the main ring. This process is likely to be required to deliver high beam intensity for the high energy fixed target program.

The colliding beam proposal requests funds for facilities necessary in an R&D program on colliding beams. This R&D program includes studies of both proton-proton collisions between the main ring and the Doubler/Saver and proton-antiproton collisions within the Doubler. This proposal also requests funds for the creation of a facility for the production of antiprotons for the proton-antiproton colliding beams. The work includes the extraction of protons from the main ring, their transport and targeting to produce the antiprotons.

The recently demonstrated techniques of electron beam and stochastic cooling have provided the possibility that proton-antiproton collisions of useful luminosity may be obtained. Since these techniques are still largely unexplored, it is difficult to predict the performance level which may be achieved. The colliding beam program involves the use of very high vacuum and of bunched beams. It appears that the non-linear interactions of bunched beams is an important complication and may seriously limit the attainable luminosity. Since the anticipated luminosities of the proposed proton-proton and proton-antiproton colliding beams are relatively low, any reduction below the projected luminosities would seriously reduce the usefulness of the facility for particle experiments.

Because of this and other uncertainties, we feel that the Fermilab colliding beam programs should be considered as an accelerator research and development effort with substantial risk but with an extremely high potential payoff for the High Energy Physics program.

SECTION IV

RECOMMENDATIONS AND COMMENTS

1. We recommend as our first priority the authorization of the BNL proposal for the construction of the proton-proton colliding beam facility ISABELLE, to operate with a maximum energy of about 400 GeV per beam and a peak luminosity of $10^{33}cm^{-2}sec^{-1}$.

2a. We recommend authorization in FY 1979 of $12.8 million for Tevatron construction (sufficient apparatus and facilities to convert the completed Energy Doubler/Saver R&D project to an accelerator and to extract a 1 TeV beam to an appropriate beam dump or target). We also recommend authorization of about $10 million in FY 1979 for the highest priority project(s) (including beam transport systems) that will enable Fermilab to begin the exploitation of the Tevatron for fixed target 1 TeV physics.

2b. We recommend that funds be provided in FY 1979 for construction of the necessary facilities and apparatus needed in support of an accelerator R&D program on proton-proton and proton-antiproton colliding beams at Fermilab.

3. In the absence of a formal request for funds (submission of Schedule 44) for the PEP energy upgrading, we chose not to make a specific recommendation on this project. We suggest that HEPAP or an appropriate Subpanel consider the matter when the Schedule 44 is available.

Comments on Recommendation 1

The experiments which can be carried out with a high energy proton-proton colliding beam facility (ISABELLE) are of great importance in extending our understanding of the fundamental properties of matter. These experiments are complex since many of the processes of interest are subtle and have low event rates. A machine of high luminosity with low background and dedicated to colliding beam work is necessary to explore fully the very high energy regime.

The 1975 Subpanel recommended construction in FY 1977 of such a device--a BNL proposal for a colliding beam facility with a maximum energy of 200 GeV per beam. In the intervening 2 years, technical advances have been made which permit a maximum energy of about 400 GeV per beam to be achieved with only a 40 percent increase in the circumference of the rings. While the physics interest of the lower energy is great, the incremental physics benefits of the higher energy are impressive relative to the incremental cost. For example, if the anticipated intermediate bosons W and Z are found with the expected properties, the unity of the weak and electromagnetic interactions will have been shown. The pressing next question will then concern the precise structure of the combined interaction. There will then be an immediate and intense interest in searching for similar particles of higher mass, which can shed light on the nature of that structure. Since these particles will probably have smaller production cross sections than the W and the Z, it is clear that not only the high ISABELLE luminosity but also the higher energy will be of great importance in carrying out such a search. On the other hand, if no W or Z is found, this most likely would imply rapid growth of the weak interaction with energy. Such behavior has highly significant experimental consequences, making the higher energy most advantageous. Thus, the higher energy is essential if we are to reap the maximum benefit from the investment in construction. Although the construction cost will be higher, we expect that the cost of detection equipment for the higher energy facility will be about the same as that required for the lower energy design.

It is considered of the greatest importance that construction of ISABELLE proceed at the fastest possible pace and that physics research with this machine start at the earliest possible moment. It would be acceptable, in this connection, that the machine be put into first operation at a reduced energy.

The ISABELLE design incorporates six interaction regions for experimental work. Two of these are also used for extraction and/or injection and may have high background levels. We are concerned that these two regions may not be as suitable for experimental purposes as the other four. Because of its expense and technological complexity, the ISABELLE facility is unlikely to be duplicated elsewhere in the world. In light of the great interest of the world high energy physics community in the use of ISABELLE, we recommend that BNL consider carefully the addition of two interaction regions to bring the total number to eight. We understand that the change in ring design from one with six-fold symmetry to one with eight-fold symmetry can easily be accomplished with a modest construction cost increase, primarily for experimental halls and beam crossing equipment.

The Subpanel notes the rapid technological progress toward generating and storing antiproton beams of sufficient intensity for proton-antiproton collisions and the importance of experiments with collisions of this kind. It is clear that ISABELLE can ultimately provide a facility for physics research with proton-antiproton colliding beams at high luminosity, low background, and long beam life.

The Subpanel also notes that the capability of electron-proton colliding beams can be added at a later time.

Comments on Recommendation 2a

The Subpanel recognizes the prime importance of fixed target physics at the highest possible energy at Fermilab in the context of the national program. The progress made at Fermilab in the development of superconducting magnets for the Energy Doubler/Saver program has been impressive and the Subpanel has confidence that with the present rate of technical progress and with adequate funding, this R&D program will be completed in a timely manner. Since Fermilab, over the next decade even more than at present, will be the primary United States facility to support a broad-based fixed target research program, it is essential that it be upgraded in the near future to operate in the 1 TeV region. The Subpanel notes that the energy of 1 TeV is more than twice that available at any other fixed target accelerator.

In view of the great promise of 1 TeV fixed target physics, the Subpanel has therefore recommended providing construction funds beginning in FY 1979 to make this physics program possible. The goals envisioned include:

(i) particle acceleration to 1 TeV with a cycle time of no more than 1 minute and an average intensity of at least 10^{12} protons per second;

(ii) accelerator operation up to 500 GeV in the energy saver mode, thereby effecting significant savings in power costs;

(iii) resonant extraction at 1 TeV, to provide a spill of up to 20 seconds;

(iv) beam transport to one or more of the experimental areas; and

(v) upgrading those areas to permit physics with the 1 TeV beams.

The Subpanel notes that a full and diverse fixed target program with high utilization would benefit substantially from considerably greater average intensity than the design goal of 10^{12} protons per second. Thus, it is urged that the magnets of the Energy Doubler/Saver be made with the goal of accepting and accelerating 10 stacked turns from the main ring. Other components needed to achieve higher intensity, such as increased RF power capability, can be added at a later date.

We believe community-wide input should be sought on the best allocation of the recommended $10 million for initial exploitation of the Tevatron for fixed target 1 TeV physics. We also urge broad user and Fermilab planning and design efforts on the remainder of the conversion of the experimental areas to a full 1 TeV research program. These efforts should culminate in a construction proposal for FY 1980.

Comments on Recommendation 2b

Completion of the Doubler/Saver ring offers an early opportunity for testing the feasibility of bunched beam proton-proton and proton-antiproton collisions.

To pursue the possibilities of experiments with very high center-of-mass energy based on colliding beams and to permit reliable estimates of colliding beam luminosities, a program in accelerator R&D must be carried out to answer questions relative to the dynamics of colliding bunched beams and, in the proton-antiproton case, to develop beam cooling techniques. The funding recommended is to provide beam transport, target facilities, and interaction regions with enlarged enclosures that are needed for these investigations and for the early subsequent experiments. These facilities should be planned to minimize interference with the laboratory's program of fixed target experiments.

Comments on Recommendation 3

The suggested increase in the center-of-mass energy of PEP is of great importance for the study of the weak interactions in a purely leptonic system, and therefore represents research of a most fundamental character. As this plan was first presented after the formation of the Subpanel and in view of the lack of detailed information, the Subpanel did not consider the PEP energy upgrading on an equal basis with the Fermilab and BNL proposals. We suggest that HEPAP or an appropriate Subpanel consider the matter when a detailed proposal is available.

SECTION V

GENERAL COMMENTS

1. Diversity and Balance of the Recommended Program

The national High Energy Physics program includes different types of accelerators each with unique capabilities, covering a wide range of energies. Recent developments have shown again how an important discovery in one area gives rise to new insights in other parts of the High Energy Physics program, and in other areas of physics. The Subpanel has taken this into account in making its recommendations, seeking to maintain a correct balance among the different parts of the program. Some reduction is foreseen in a few years in the program for the lower energy regions, but new kinds of low energy experiments will continue to take place and may be of vital importance. The largest activity during the next decade should be in the evolving frontier programs with PEP, the Tevatron program and ISABELLE.

2. Medium Term Planning

An important capability which will be needed is a colliding beam facility for electron-proton collisions. This should be an incremental installation at an existing facility. Studies with electron-proton collisions combine the clean experimental conditions and ready theoretical interpretation of experiments with a lepton probe, with the high center-of-mass energies which can be obtained with colliding beams. These studies should lead to significantly greater fundamental understanding of the structure of protons and the interrelation of weak and electromagnetic forces. This project should be pursued as soon as it is technically appropriate.

We also foresee the possibility of smaller construction projects in this period, such as the addition of a proton-antiproton collision capability to ISABELLE and the construction of a bypass section on the Tevatron to allow efficient use of colliding beams.

3. Long-Term Planning

The construction program outlined above has been designed to preserve sufficient funds within the base level construction guidelines for a start on the next large high energy facility, towards the end of the 10-year planning period. The particular type of machine (multi-TeV fixed target or colliding proton beams, electron-positron colliding beams at higher energies) which will be required to best advance our knowledge of high energy physics beyond the end of the planning period cannot be determined yet. Some of the above facilities might require a larger site than is currently available at the existing sites.

The importance to the health and strength of the national High Energy Physics program of being able to open new experimental frontiers is indeed clear. A substantial part of the program funding will always have to be devoted to construction of new facilities. New technological advances permitting us to maintain the thrust to high energies, to reduce the cost per GeV, and to improve detection capabilities will be decisive for the field.

In view of the rapid advances being made in accelerator technology, as well as the compelling physics reasons for pursuing the study of higher energy interactions, the Subpanel urges that a strong program of research and development work directed toward continued major improvements in accelerator and detector technology be carried out. For example, higher field superconducting magnets and improved superconducting RF cavities would allow substantial cost savings for future higher energy machines.

4. International Aspects

In the past the field has benefited from the existence of similar accelerators in Western Europe, the Soviet Union, and the United States. This has provided for cross-checking of significant results and has also added to the breadth of the international program through the different physics emphasis of the various laboratories. With the construction of ISABELLE, a device which is likely to remain unique in the world, we will be moving into an era where the cost and complexity of the new accelerators will make some coordination of programs in different regions advisable.

In order to preserve the breadth and vitality of high energy physics it is thus even more desirable than in the past to assure that major new facilities built in the United States and abroad be easily accessible to the entire international high energy physics community. The particular experiments which are performed at these facilities should be determined on the basis of scientific merit and feasibility.

The Subpanel further recognizes that future major increases in the scale of accelerator construction may eventually require cooperative funding by the United States and other countries. However, we have no expectation of any fully international accelerator in the next decade. Therefore, other than recognizing the possible need for United States participation in design studies toward the end of the coming decade, further consideration of an accelerator built under international auspices was not regarded as being within the scope of the Subpanel's charge.

SECTION VI

EFFECTS OF ALTERNATE FUNDING CONSTRAINTS

1. Base Budget Case

The base budget constraint is the same as that given to the 1974 and 1975 Subpanels; namely, that the average annual operating and equipment funding over the next decade is to be about the same in constant value dollars as that for the reference period from FY 1965 through FY 1974, and the average annual construction funding over the next decade is to be about two-thirds of that in constant value dollars for the reference period from FY 1960 through FY 1974.[1]

Implementation of the FY 1979 recommendations and the medium term construction that we have considered for this decade will make possible a strong and exciting program of High Energy Physics focused on the new facilities. The Subpanel was careful within the constraints to preserve the funding needed to start construction on a major new facility late in the decade as discussed in Section V. However, the funding constraints of this case will require further curtailment below present levels of the productive and valued programs at existing facilities. In addition, new experimental equipment designed to take advantage of advancement in detector technology will have to be deployed at a less than optimal rate.

2. High Budget Case

The upper budget level given to the Subpanel provides an average of $10 million of additional annual construction funding and an average of 5 percent of additional annual funding for operating and equipment for the period under consideration. The relatively small percentage increase in funding permits a substantially improved program over the base case.

The increased equipment funding under this case would allow a buildup of equipment and associated technical support at the new facilities which would better match the opportunities provided by these facilities. In addition, the new construction funds would allow the new facilities now under construction and those recommended for FY 1979 construction to proceed at a less restricted pace and with an initial utilization better matched to their physics potential.

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[1] This constraint, in FY 1978 dollars, allows an average of $290 million annually for operating and equipment and an average of $60 million for construction for the decade from FY 1978 through FY 1987.

With the higher funding level the utilization of the richly productive lower energy programs at BNL, Cornell, Fermilab, and SLAC would not have to be curtailed as in the base case and could be maintained at a good level.

Finally, the improved funding would permit an earlier start on R&D and on initial construction for the next major facilities discussed earlier under medium and long range plans.

3. Low Budget Case

This funding level is approximately equal to "Level B" (after correcting for inflation) analyzed in the 1975 Subpanel report, and is about the same as the FY 1978 budget now before Congress.[2] Even under this level of funding, we cannot abandon any of the three main thrusts toward higher energy--electron-positron colliding beam rings, proton-proton colliding beam rings, and high energy fixed target work. These new capabilities are essential to retain the cutting edge of this important area of science. However, stretch-out in attaining these goals will be required.

The consequences of this funding level on accelerator operations, and particularly on the lower energy program, would be severe. The utilization levels would be low. The impact of the expected PEP turn-on in FY 1980 and the pressure for increased operation (particularly at Fermilab) already have forced the planning of the phase out of the Zero Gradient Synchrotron. Initiation of the PEP and the Fermilab Tevatron programs will exacerbate the situation.

The addition to the program of a major proton colliding beam machine, ISABELLE, will make a difficult situation more difficult. Nevertheless, we believe such a facility is necessary though its construction schedule would be painfully slowed. We also believe

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2/ The low budget case allows for $256 million per year in FY 1978 dollars in operating and equipment funds, and $46.5 million per year in construction funds over the next decade.

that R&D work on future accelerators such as electron-proton colliding beams, very high energy electron-positron colliding beams, and multi-TeV fixed target or proton-proton colliding beams must continue; for, a start on one of these projects will still be required before the end of the decade. The start on construction of this facility would be delayed until the end of the decade.

The consequences to the program would be a drastic reduction or termination of the lower energy fixed target and colliding beam programs, and a reduction in the level of support and number of university groups. In addition, levels of use of the new electron colliding beam facilities, of Fermilab, and of the proton colliding beam facility will only be at about the 50 percent level. Further, the provision of equipment necessary to exploit these facilities will be substantially slowed. This funding level on a long term basis is close to that which would require a drastic revision in the long range program and probably is not adequate to keep the United States in a world competitive position in this field.

APPENDIX A

CHARGE TO 1977 SUBPANEL ON NEW FACILITIES

Consider the future needs of the United States High Energy Physics program in a long range context and make specific recommendations with respect to FY 1979 construction items. The study is to be done under the following funding constraints:

1. A base case developed under the same funding constraints as those given to the two previous Subpanels; namely, assume the average annual operating and equipment funding over the next 10 years to be about the same in constant value dollars as that for the decade from FY 1965 through FY 1974, and the average annual construction funding over the next decade to be about two-thirds that for the period from FY 1960 through FY 1974. These funding levels in FY 1978 dollars, which include both NSF and ERDA support, are $290 million for operating and equipment and $60 million for construction.

2. Consideration is also to be given to the effects of two alternate funding levels, one lower than the base and the other higher. The lower level should assume an average annual funding level in constant value dollars about equal to that currently in the budget for FY 1978, namely $256 million for operating and equipment and $46.5 million for construction. The higher level is to be based on an average annual level for operating and equipment 5 percent higher than the base level, namely about $305 million, and an annual construction level of $10 million more than the base level, namely $70 million.

APPENDIX B

PARTICIPANTS
1977 SUBPANEL ON NEW FACILITIES

Panel Members:

- *J. Sandweiss (Yale)
- B. Barish (Cal Tech)
- J. Bjorken (SLAC)
- T. H. Fields (ANL)
- H. Frisch (Chicago)
- G. Lambertson (LBL)
- L. J. Laslett (LBL)
- **J. E. Leiss (NBS)
- ***F. E. Low (MIT)
- **B. McDaniel (Cornell)
- H. Neal (Indiana)
- P. Reardon (Princeton Plasma Physics Lab)
- B. Richter (SLAC)
- J. Rosner (Minnesota)
- W. Schnell (CERN)
- G. Trilling (LBL)
- W. Willis (CERN)
- R. Fricken (ERDA), Executive Secretary

Consultants:

- R. Kropschot (NBS-Boulder)
- M. McAshan (Stanford)
- A. McInturff (BNL)
- M. Month (BNL)
- J. Simpson (ANL)
- L. C. Teng (Fermilab)
- A. Tollestrup (Fermilab)

Observers and Visiting Participants:

BNL Representatives:

- M. Barton
- H. Hahn
- T. Kycia
- R. Palmer
- R. Rau
- J. Sanford
- N. Samios
- G. Vineyard

Fermilab Representatives:

- D. Cline (Wisconsin)
- T. Collins
- J. Cronin (Chicago)
- W. Fowler
- E. Goldwasser
- F. Huson
- P. Livdahl
- P. McIntyre (Harvard)
- J. Peoples
- N. Ramsey (URA)
- R. Wilson

SLAC Representatives:

- J. Ballam
- H. Wiedemann

Other:

- M. Bardon (NSF)
- S. Drell (SLAC, HEPAP)
- D. Gould (MIT)
- J. Kane (ERDA)
- H. Kinney (ERDA)
- D. Sutter (ERDA)
- W. Wallenmeyer (ERDA)

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*Chairman

**Co-Chairman, Accelerator Subgroup

***Chairman, Physics Subgroup

THE U. S. ENERGY RESEARCH & DEVELOPMENT ADMINISTRATION LONG TERM STRATEGY FOR CONSTRUCTION & OPERATION OF HIGH ENERGY PHYSICS FACILITIES

OCTOBER 10, 1975

UNITED STATES
ENERGY RESEARCH AND DEVELOPMENT ADMINISTRATION
WASHINGTON, D.C. 20545

October 10, 1975

Mr. Hugh F. Loweth
Deputy Associate Director for
Science, Space, and Energy Technology
Office of Management and Budget
New Executive Office Building
Washington, D. C. 20503

Dear Mr. Loweth:

In response to your memorandum of April 9, 1975, to Dr. Seamans, I am pleased to transmit herewith a report entitled "Long-Term Strategy for Construction and Operation of High Energy Physics Facilities."

After a brief discussion of the nature, goals, and trends of high energy physics research, the report develops a facility posture for high energy physics in 1985 which meets the major scientific needs of the program as foreseen today. The posture is developed under the assumption that future funding for high energy physics may be constrained to a level below that in the approved ERDA program requirements projections. We believe that the posture developed will serve as a useful guideline for program decisions over the next few years. The report also assesses the impact of several alternative plans as agreed in our discussions. Finally, the report addresses the subject of agreed-on long range planning and budgeting for high energy physics.

We are available to discuss the report with you and your staff.

Sincerely,

/s/

John M. Teem
Assistant Administrator for
Solar, Geothermal and
Advanced Energy Systems

Enclosure:
Report

Digest

OMB Request

This report is in response to a request from the Office of Management and Budget (OMB) for a study of certain aspects of High Energy Physics (HEP). The main issue raised in the request is: "What should be the long-term strategy toward the construction and operation of HEP facilities?" Sub-issues include:

- A long range (1985) facility posture for high energy physics.
- Priorities of proposed major construction projects.
- Impact of alternative funding levels.
- Effects of the lack of agreed-on long range planning and budgeting.

Goals and Status of High Energy Physics

The goal of High Energy Physics is the understanding of energy and matter in their most basic forms. It examines transformations and interactions among the ultimate constituents of matter and searches for new laws of nature. The possibility of breakthroughs in this field has seldom been as great as it is today. Excitement is at an all-time high as recent discoveries, many of them made in the U.S., indicate that a new level of insight into the structure of matter and a more profound understanding of the forces of nature is close at hand. Evidence from experiments at every major accelerator, in combination with theoretical advances, all point to a new substructure of matter, and to a synthesis of basic forces that, up to now, have been regarded as being completely distinct from one another. This synthesis is the same kind as Maxwell achieved for electricity and magnetism and Einstein for matter and energy.

Future Research Directions

The viability and productivity of High Energy Physics depend critically on the timely availability of new technological capabilities. It is essential in such a field which explores the frontier of scientific knowledge, to be able to follow the directions indicated by the research and at a pace which exploits new discoveries. Past experience in high energy physics has shown that the investment of about 20 to 25 per cent of the total Program support in new facilities provides a good balance between the introduction of new capabilities at a rate matched to the advance of the science, and the continuation of research best done with the existing accelerators.

Recent progress has shown that the information gained from using various lepton and hadron probes is becoming increasingly interdependent over a broad range of energies. The experimental results from one type of facility are having an increasingly large impact on the design and scheduling of experiments at other facilities.

Colliding beam devices, a relatively recent development, have an overriding advantage for reaching very high interaction energies. All of the energy of the colliding particles can go into the interaction, whereas in a fixed-target accelerator most of the energy of the incident particle goes into forward momentum of the collision products.

Colliding positron and electron beams produce matter-antimatter annihilations into pure energy and thus provide the source of an initial state of particular simplicity, which may then materialize into new forms of matter. This device is an ideal tool to look for and study new particles, as the recent discoveries at SPEAR attest.

Proton-proton colliding beams provide reaction energies in a range which would be prohibitively expensive if obtained by any other means. The basic characteristics of the strong force can be studied at the highest energies with this tool. Such a capability in the hundred GeV range promises also to be an effective way of observing new manifestations of weak interaction effects. With such a machine, however, secondary beams cannot be produced and the interactions studied are restricted to those between the two beams of protons.

A fixed-target proton accelerator, while not attaining as high an effective reaction energy as a colliding beam machine, has a much higher interaction rate since the fixed-target consists of dense matter rather than a diffuse beam of counter-rotating particles. Intense secondary beams of various particle types otherwise unobtainable (π-mesons, K-mesons, neutrons, antiprotons) can be produced and used for further studies. Additionally, the meson beams can be used to produce tertiary beams of high energy neutrinos and muons.

Concurrent with the need for new facilities is the need for a continuing and effective research utilization of existing facilities. In recent years concern has increased about the utilization levels of the high energy accelerators which are currently about 50 per cent. The utilization levels have decreased due to inflationary effects and the needs associated with bringing a major new laboratory (FERMILAB) into operation. Care has to be given in long range plans to achieving a reasonable balance between utilization of existing facilities and provision of new facilities.

Long-Term Posture

The development of the long-term (1985) facility posture has drawn heavily on the deliberations of the 1974 and 1975 Subpanels on New Facilities of the High Energy Physics Advisory Panel (HEPAP) and an extensive history of considerations by HEPAP. This posture assumes the funding guidelines provided to the 1974 Subpanel, which correspond to Level I in this report. This permits an annual level of effort for the national program somewhat below the level of effort during the FY 1965 to FY 1974 ten year period, and also below the level represented by the current ERDA five-year planning projections for high energy physics. The new facilities in the posture are based on the three-pronged approach to higher energy which was strongly and unanimously recommended by the two Subpanels and by HEPAP.

Progress in basic research inherently follows a course which is continually being modified by the impact of the most recent developments. The plans developed in this report would provide for the high priority needs of the Program as seen now. New scientific and technical knowledge will undoubtedly dictate, at a later date, modifications of the plans and the facility posture presented here.

The principle considerations and assumptions used in developing the long-term facility posture include the following:

1. High energy physics is a forefront fundamental science in which it is important that the U.S. maintain a leading role.
2. It is essential for a science at the frontiers of the unknown that a substantial part of the program effort be devoted to the provision of new and innovative facilities.
3. The increasing interdependence of the research done with different particle probes mandates a three-pronged approach to higher energies via positron-electron colliding beams, proton-proton colliding beams, and protons colliding with fixed targets.
4. Firm funding constraints will remain on the program. The average annual funding assumed for the next ten years for the national program (ERDA and NSF) is $245 million per year in FY 1976 dollars for operating and equipment, which is about the same it was for the FY 1965-74 period; and $50 million per year in FY 1976 dollars for construction, which is about two-thirds the average annual level for FY 1960-74.
5. Thus, U.S. funding will continue to trail that for Western Europe, currently about two and one-half times greater.
6. In providing new physics capabilities, maximum advantage will be taken of current assets of the Program, including the major accelerators, and support facilities (all in forefront condition) and an outstanding cadre of highly skilled scientific and technical manpower.
7. The continuation and enhancement of international cooperation and collaborations are important for U.S. progress and success in high energy physics and will be vigorously pursued.
8. The cost of research in high energy physics exerts a strong influence on the facility posture of the field. Scientific redundancy which is important for verification purposes will largely be possible only on an international basis.

The 1985 U.S. High Energy Physics Program would then include:

1. Facility utilization levels of about 70 to 75 per cent.
2. A 15 GeVx15 GeV positron-electron colliding beam facility (PEP)

operated jointly by SLAC and LBL and using the present SLAC linear accelerator as an injector.

3. Higher energy electron (and positron) beams on fixed-targets at SLAC resulting from improvements currently underway (SLED). By 1985 the SLAC linear accelerator would be operating primarily in the 20 to 45 GeV energy range for electron fixed-target physics.
4. A 200 GeVx200 GeV proton-proton colliding beam facility (ISABELLE) in operation at BNL, using the AGS as an injector.
5. An AGS proton fixed-target program up to 33 GeV with broadened emphasis on lower energy secondary beams in partial compensation for the loss of the ZGS once it is shut down (perhaps in FY 1981).
6. A FERMILAB facility which would have had substantial growth in experimental areas and capabilities. Some high energy proton fixed-target capability at 800 to 1,000 GeV would probably be available based on the Energy Saver/Doubler which is assumed to have been successfully developed and put into operation about 1979.
7. A multi-TeV fixed-target proton accelerator and/or colliding beam facility would be nearing completion at FERMILAB.

Plans and Priorities

The long-range plans and priorities for high energy physics outlined here exploit existing facilities through their productive lifetimes and make an efficient transition from present to future facilities. They are designed within a constrained funding total to maintain the vigor and productivity of the U.S. High Energy Physics Program, and a competitive world position. High priority is given to improving utilization to the 70-75 per cent level. The interdependence of research with the electron-positron colliding beams, the proton-proton colliding beams and the proton fixed-target machines mandates movement to higher energy on all three. Timewise, however, the first priority for new facility construction is the positron-electron project, followed by the proton-proton colliding beam device.

Based on Congressional action to date construction of PEP is planned to begin in FY 1976 with initial operation for research in FY 1980. Construction of ISABELLE would begin in FY 1978 with initial operation in 1983 and would give the U.S. its first proton-proton colliding beam capability. The third major component of the 1985 posture is a multi-TeV proton fixed-target accelerator with beam energies of at least 2 TeV and as high as 5 TeV. FERMILAB is an obvious choice for its location. Construction could begin about FY 1980 and it would then be available for research shortly after 1985. The long-range plan for construction also includes accelerator improvement projects (AIP) at a level of about $8 million per year. Figure 4, page 28, graphs the history of construction for the Program since FY 1960, and projects the funding required for the next decade to implement the program.

During the next few years the utilization levels of the AGS, FERMILAB, SLAC, and the ZGS would be increased from the present levels of about 50 per cent to about 70-75 per cent. This would then be maintained, although there would be a continuing evolution in the character of the program resulting from the phasing out of some efforts and the phasing in of others, as the physics advances and as new capabilities become available. SPEAR would be turned off as PEP comes into operation, the ZGS would be turned off as the build-up for ISABELLE operation begins, the AGS would shift emphasis to include lower energy physics after tne ZGS is shutdown, SLAC would concentrate on the 20-45 BeV range and FERMILAB would focus on the higher energies made possible by the Energy Saver/Doubler.

High priority would be given during the next few years also to the development of the Energy Saver/Doubler at FERMILAB. This project, when successful, would save a major fraction of the FERMILAB power needs, reduce operating fund needs and increase the peak energy and duty factor of the accelerator. The superconducting magnet and cryogenic development efforts are an important element for the Multi-TeV facility which would follow.

By 1985, FERMILAB, AGS, and SLAC would be the only major accelerator centers for high energy physics in this country. Each of them, however, would be on a unique forefront of the field and would complement the programs at the other two laboratories. The support required for user groups, mostly from universities who plan and carry out the experiments, remains a constant fraction of the operating expenses.

Figure 5, page 31, shows the projected distribution of operating funds through FY 1985. Capital Equipment funds, required to provide the apparatus by which new discoveries can be made, are an important element of the plan.

The long range equipment funding levels are shown in Figure 6, page 33. These derive from the need for continuing programs at the existing facilities, incremental needs for initial complements of equipment at new facilities, and the continuing needs at new facilities as they become established.

Alternative Funding Levels

Consideration of the effects of four funding constraint levels were agreed to in discussions with the OMB. These levels are listed and discussed below. In each case the constraints refer to the average annual level of effort over the FY 1976-85 period and include total support for the program from all Federal sources. All funds are expressed in FY 1976 dollars.

Level I

This is defined as:

1. Operating and equipment. $245 million per year. Same as for FY 1965-74.
2. New facility construction. $50 million per year. About two-thirds that for FY 1960-74.

The posture and plans discussed earlier were structured to meet the constraints of this level, which is below the average level of the U.S. program in FY 1965-74, and well below the funding for the European high energy physics program. Nevertheless, it represents a good program which would maintain a strong and leading role for the U.S. It provides for reasonable utilization of operating facilities and the timely introduction of new facilities.

Level II

This is defined as a level close to optimum for the program. Included in this level would be the additional construction of the proposed Cornell 8 GeV electron-positron colliding beam facility; other colliding beam capabilities at ISABELLE and/or PEP, such as proton-antiproton, and electron-proton; a superconducting stretcher ring at the AGS; enhanced AIP funding; and additional beam lines, experimental areas and large detection systems. Also included would be higher utilization levels; more rapid development of the Energy Saver/Doubler; and a higher R&D level of effort for instrumentation and detectors. Due to the complexity assessing the detailed structure of the program at this level, a HEPAP Subpanel has been set up to study it and submit a report in six to eight months.

Level III

This is defined as:

1. Operating and equipment. $190 million per year. Same as FY 1976.
2. Construction. $20 million per year.

This level would clearly result in a loss of U.S. leadership in the field. Only one of the new facilities (PEP) could be completed prior to 1985 and only one of the remaining two could be attempted and completed in the next fifteen years. Utilization would be no more, or less than the present 50 per cent levels, and during the early part of the period would fall to near 40 per cent to permit developing the Energy Saver/Doubler and bringing it into being. The future of high energy physics in the U.S. would be placed in question and the advancement of the science worldwide would be slowed.

Level IV

This is defined as about midway between Levels I and III.

This level would permit sticking to the three-prong approach to new facilities, however the third step would be substantially stretched out and delayed. The Energy Saver/Doubler could be adequately pursued; however, there would be little improvement in present utilization levels and substantial components of ongoing programs would have to be eliminated as new facilities come into operation. Initial complements of equipment for the new facilities would be reduced.

Long Range Planning and Budgeting

The inherent long time scales associated with high energy physics, the relatively high costs of large accelerators and their associated facilities, and the need for a continual infusion of new technological capabilities, are all features which demand careful long term planning. The lack of agreed-on long term budgeting in the past has caused inefficiencies and some damage to the program. Permission for flexibility for operating funds to be overrun or carried over between fiscal years (as little as 2%), would enhance the ability of laboratories to respond to late program changes and would significantly increase efficiency of the program.

An example of the successful implementation of longer range budgeting is provided by the "Bannier Procedure" which is used at CERN. Firm budgets for a given fiscal year are approved one full year in advance and provisional budgets are approved for an additional two years. Each is in funds of constant purchasing power, and the actual funding is adjusted as necessary be changes in the price index. A similar procedure would make a significant contribution to the effectiveness of the U.S. program.

I. Introduction

A. OMB Request

This report is in response to a memorandum (Loweth to Seamans, April 9, 1975, see Appendix A of this report) from the Office of Management and Budget (OMB) to the Energy Research and Development Administration (ERDA) on the subject of High Energy Physics (HEP). The main issue raised in the memorandum is: "What should be the long-term strategy toward the construction and operation of HEP facilities?" The memorandum also indicated three sub-issues to be addressed:

1. To what extent, if at all, has the lack of agreed-on long range planning and budgeting for HEP resulted in inefficient use of available facilities and other resources? How would the present yearly review process be improved by longer range plans based on alternative funding scenarios?

2. What should be the long range (i.e., 1985) facility posture of the U.S. in HEP? How will this posture be affected by the high cost of HEP research and by the international nature of HEP research?

3. In the light of the results of the above issues, what are the priorities of the proposed major construction projects?

At a subsequent meeting of the OMB and the Division of Physical Research (DPR) of ERDA, it was agreed that the impact of four alternative levels of funding should be looked at for the projected FY 1976 to FY 1985 ten year period. The four levels agreed to for the national program (ERDA and NSF) were:

1. A level corresponding to the constraints given the 1974 Subpanel on New Facilities, i.e.:
    a. An average annual operating and equipment level about the same as that for the FY 1965 to FY 1974 period. (About $245 million per year in FY 1976 dollars.)

   b. An average annual construction level about two-thirds that for the FY 1960 - FY 1974 period. (About $50 million per year in FY 1976 dollars.)

2. A level close to optimum for the program.

3. A level for operating and equipment corresponding to the FY 1976 funding level. (About $190 million per year) plus $20 million per year for new construction.

4. A level about half way between 1 and 3 above.

B. Response

The goals of the High Energy Physics' Program, its recent history and present status are described briefly to provide the context for discussion of a long-term strategy for construction and operation of high energy physics facilities for the United States. The research progress and technological developments of the past decade provide the bases for determining what facilities will be needed for continued scientific advances in the next decade.

The scientific justification and technical feasibility of several specific construction projects have been studied within a general long range program context by two Subpanels on New Facilities of the High Energy Physics Advisory Panel (HEPAP), which met in the springs of 1974 and 1975 respectively. The long range plans and priorities of this report have drawn heavily from the deliberations of these two Subpanels and from an extensive history of study and discussions with HEPAP.

The long term facility posture, plans, and priorities discussed in greatest detail here were developed using the same funding constraints provided to the two Subpanels on New Facilities for their guidance in forming priorities. These constraints represent an overall level of effort for the national program for the next ten year period somewhat below that of the FY 1965 to FY 1974 ten year period and below that represented by the FY 1976-FY 1980 Spring Planning Projections. The effects of alternative levels of funding on the program are discussed as variations from the posture and plans developed for the principal constraint level.

The natural time factors which make long range planning essential in high energy physics are outlined, and the potential benefits that would be derived from longer range budgeting are discussed. The concurrent need for flexibility in the long range plans and budgeting is also discussed.

Major research opportunities and directions for new explorations of the fundamental nature of matter and energy have resulted from the sequence of recent discoveries in the field. It is a time of many exciting discoveries and outstanding progress. The research achievements, coupled with new technological developments in the areas of colliding beams, superconductivity and detection methods, dictate the planning for the research of the 1980's.

The plans and posture described in this report reflect careful judgment as to the best courses to follow within the indicated constraints. Although consistent and within the guidelines of the approved ERDA Program Requirements Projections, the plans are not intended to represent a request. The discussion of the long range budgeting question was developed by the High Energy Physics staff of the Division of Physical Research and does not represent an official Agency position.

II. Goals and Status of High Energy Physics

The goal of high energy physics is the understanding of energy and matter in their most basic forms. The program examines the transformations and interactions among the ultimate constituents of matter, searches for new fundamental laws of nature, and seeks to better understand the established laws of nature. The justification for this effort is broadly based, ranging from a primary role in the effort of man to understand the universe in which he finds himself, through the promise of important discoveries for the future needs of society, to the potential for technological contributions to solutions of current energy problems. A deeper comprehension and understanding of the laws of nature has always rewarded man with a larger measure of control over nature for his own needs and purposes.

The significance and successes of the U.S. High Energy Physics Program have been widely recognized. The 1972 National Academy of Sciences report*, Physics in Perspective, considered 69 different "program elements" (major research areas) from the nine subfields of physics, and ranked them in terms of their overall scientific importance, and promise. Five of the top eight programs were elements of the High Energy Physics Program. The General Advisory Committee noted, in its long range research review in March 1974, that "High Energy Physics is the most advanced frontier of physical science," and that "This is the area in which the nature of matter and energy is most deeply probed.....it could yield knowledge leading to the most radical future advances, now unforeseen, in our control and use of energy." The Joint Committee on Atomic Energy (JCAE), in its FY 1972 Authorization Report, remarked that the high energy physics laboratories "provide the fundamental knowledge upon which many other basic advances in science and technology depend." They added that high energy physics "is a highly competitive field among nations as evidenced by the advanced machines in operation or under construction in the Soviet Union and Europe. It is, of course, in the national interest that the United States remain in the forefront of this field of scientific endeavor."

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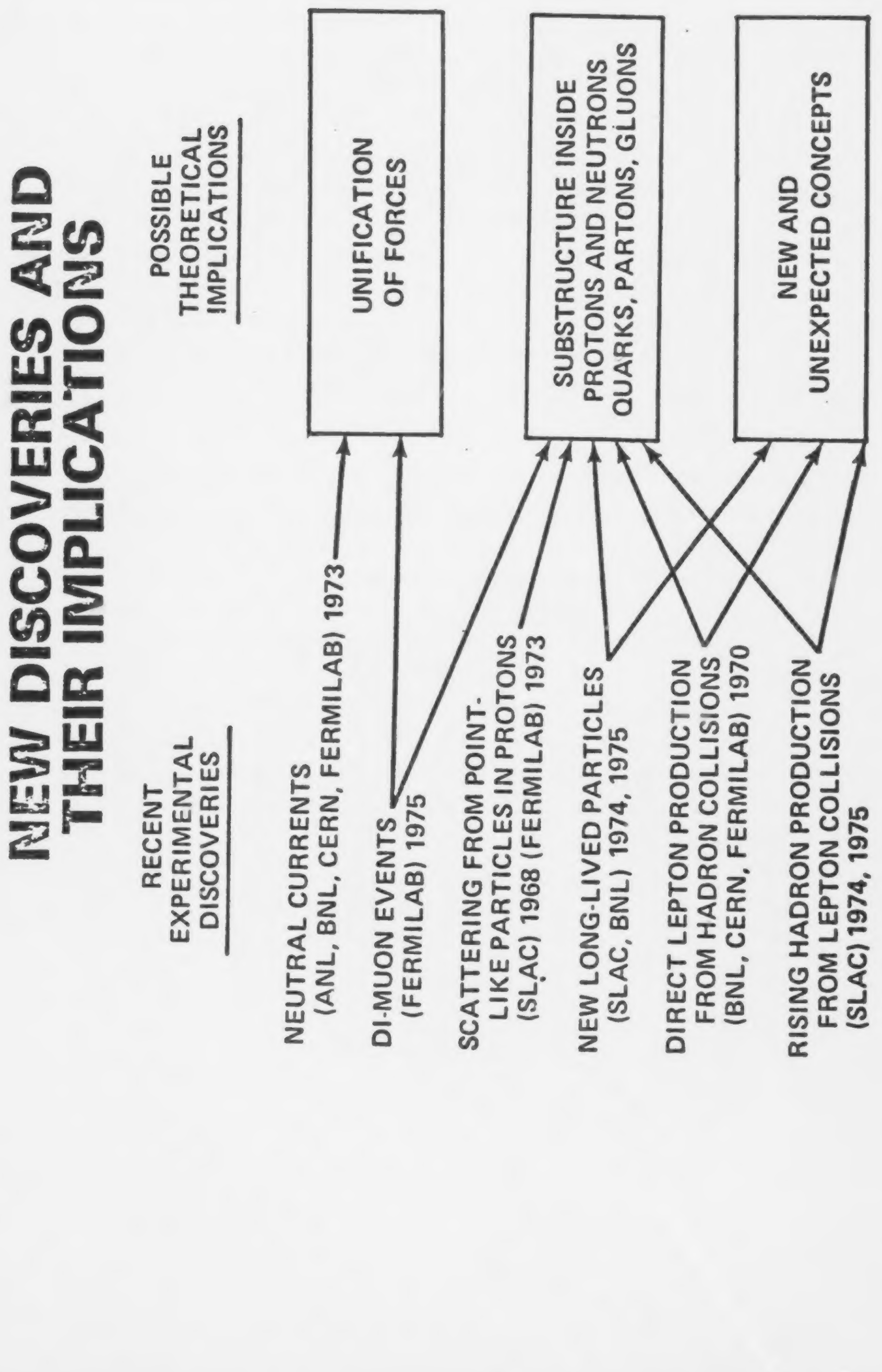
*The Physics Survey Committee which prepared the report and made these ratings consisted of 17 leading U.S. scientists (including two high energy physicists) from universities, laboratories, and industry.

The past year has been particularly productive of major discoveries in high energy physics. A new form of matter has been discovered, of which the Ψ-J particles are the first manifestation. Their masses lie in a new range, between three and five times the mass of the proton. Their properties indicate the existence of additional elementary subunits of matter, or quarks, in addition to the three which were originally postulated over eleven years ago, and which have been highly successful in explaining a myriad of experimental observations. An unexpectedly high rate of hadron creation in the annihilations of electrons and positrons at SPEAR (the 4 GeV colliding beam device at SLAC) also suggests the existence of a fourth quark. Experiments this year with high energy neutrino beams strongly imply still other new forms of matter, possibly related to the Ψ-J particles.

High energy physics studies the fundamental forces of nature which govern the structure and behavior of matter and energy. Four basic forces of nature are known. The strong nuclear force, dominant inside the atomic nucleus, determines the structure of nuclei and the energy released during their fission and fusion. The electromagnetic force, the best understood of all the forces, acts between charged particles and is the basis of electromagnetic radiation, the laws of chemistry, and the structure of the macroscopic world. The weak nuclear force is dominant in the interactions of neutrinos with other forms of matter, and also governs the radioactive decay of unstable nuclei. The fourth and weakest of the known forces, gravitation, determines the structure of matter on the solar and astrophysical scale.

The possibility of breakthroughs in this field has seldom been as great as it is today. Evidence coming in from experiments at every major accelerator, in combination with the theoretical research efforts of the program, seem to point to an underlying unity among the forces of nature and to new substructure within the known elementary particles. (See Figure 1) There is increasing evidence for a unification of the electromagnetic and weak forces and there are hints that the strong force also may be part of this synthesis. If, in the next generation of experiments, such a synthesis turns out to be true, what would its importance be? Where would it lead? At the moment, there are no answers to these questions. However, one can look back in history and see the world-changing effects following the unification of the electric and magnetic forces as represented by the equations of Maxwell; and likewise the world-changing consequences of the unification of matter and energy as formulated by Einstein. Certainly, if a new source of energy is to be found, it will be based on the fundamental forces of nature which are a principal concern of high energy physics research.

Figure 1



NEW DISCOVERIES AND THEIR IMPLICATIONS

RECENT EXPERIMENTAL DISCOVERIES

POSSIBLE THEORETICAL IMPLICATIONS

NEUTRAL CURRENTS (ANL, BNL, CERN, FERMILAB) 1973

DI-MUON EVENTS (FERMILAB) 1975

SCATTERING FROM POINT-LIKE PARTICLES IN PROTONS (SLAC) 1968 (FERMILAB) 1973

NEW LONG-LIVED PARTICLES (SLAC, BNL) 1974, 1975

DIRECT LEPTON PRODUCTION FROM HADRON COLLISIONS (BNL, CERN, FERMILAB) 1970

RISING HADRON PRODUCTION FROM LEPTON COLLISIONS (SLAC) 1974, 1975

UNIFICATION OF FORCES

SUBSTRUCTURE INSIDE PROTONS AND NEUTRONS QUARKS, PARTONS, GLUONS

NEW AND UNEXPECTED CONCEPTS

III. Future Research Directions

A. New Facility Needs

The viability and productivity of the High Energy Physics Program depend critically on the timely availability of new capabilities. It is essential, in a field which explores the frontier of scientific knowledge, to follow the directions indicated by the results of the research and at a pace adequate to exploit discoveries and maintain momentum. Advancement is controlled by the ability to pursue discoveries at the limits of technological capability. To be effective in this field of international interest, it is important to maintain a competitive posture vis a vis other world participants. It is also important, in an absolute sense, that the field move at a pace which permits major discoveries in the professional lifetimes of the participants and permits younger scientists to take part in new developments. To do otherwise causes the field to grow old with its captains. Past experience in high energy physics has shown that the investment on the average of about 20 to 25 per cent of the total program support in new facilities provides a good balance between the introduction of new capabilities at a rate matched to the advance of research needs and the parallel need to continue that research which is best done with the existing accelerators.

The importance of new technological capabilities and new techniques to the program is illustrated by the following examples of recent events. In the summer of 1974, CERN announced the discovery of "neutral currents," a new kind of interaction between neutrinos and matter. This new phenomenon had been predicted by certain unified theories of the weak and electromagnetic interactions and its experimental observation generated a great deal of excitement among high energy physicists all over the world. Since neutrinos have the property that their probability of interacting with matter increases with their energy, and FERMILAB can produce neutrinos one-hundred times as energetic as those from the CERN Proton Synchrotron, FERMILAB quickly assumed the lead in exploring the properties of the "neutral currents."

In November 1974, the Ψ-J particle with mass of 3.1 GeV was discovered simultaneously by an MIT/BNL collaboration using a new and sophisticated detector at the AGS, and by an LBL/SLAC research group at SLAC, using the new SPEAR electron-positron colliding beam device. The unusual properties of this particle and the unexpectedness of its discovery puzzled and excited the high energy physics community. Within a few days, a second new particle with similar unusual properties was discovered at SPEAR and the existence of the first particle was confirmed at the European high energy electron-positron colliding beam facilities at DESY and FRASCATI. Since then, activity has been high at both SPEAR and DESY in following these discoveries. (The FRASCATI energy was barely enough to produce the first particle and could not effectively participate in its follow up.) Most of the information about new members of the Ψ-J family has been discovered at SPEAR. This is largely a result of better operation and higher intensity of the SPEAR device than the DESY colliding beams device, which was more recently completed and is not yet operating at full capability. On the other hand, another member of the new Ψ-J family was first discovered at DESY, primarily as a result of a sophisticated detector having a capability to detect gamma rays which the SPEAR detector did not. The SPEAR colliding beam facility has also discovered other new and extremely interesting phenomena since its initial operation, such as the mu-e events and the large hadron to muon ratio, well ahead of DESY with its slower operation.

Two lessons can be learned from these examples: First, new technological capabilities and techniques usually lead to the discovery of new physical phenomena; and second, these new capabilities enable the researchers to seize the initiative in following up new discoveries even when made elsewhere.

B. Interdependence of Probes

Recent progress has shown that the information gained from using various lepton and hadron probes is becoming increasingly interdependent over a broad range of energies. Thus, for example, exploration of hadron production is pursued at SPEAR using colliding electron-positron beams, and the structure of hadrons is probed with electron beams at SLAC as well as with hadron, muon, and neutrino beams at the ZGS, the AGS, and FERMILAB. The inverse process, the production of leptons from hadrons, is explored using proton-proton colliding beams at the CERN Intersecting Storage Rings (ISR) and proton beams on fixed targets at the AGS and at FERMILAB. The relationships among the different accelerators, using different particles as probes, and in different energy regions are also illustrated by results from SLAC on electron-proton scattering which led directly to the concept of energy scaling; a concept which predicts that the scattering probability is a particularly simple function of energy. Subsequent tests of this concept at BNL have shown that the muon-proton and electron-proton results are similar. Now the scaling concept is being tested at higher energies in neutrino and muon-proton experiments at FERMILAB, where preliminary results indicate a breakdown in scaling due to the appearance of new processes at these higher energies. The results of the SLAC and BNL studies of the structure of the proton are also related to the results of entirely different types of experiments at FERMILAB and the ISR which study proton structure by large angle proton-proton scattering. Figure 1 demonstrates that a large number of recent discoveries associated with different facilities around the world bear upon a few central problems of high energy physics. As the research proceeds to higher energies, where it appears that the strengths of the different types of interactions will become comparable, the experimental results obtained from one facility will have an even greater impact on the design and scheduling of experiments at other facilities.

C. Advantages of Various Types of Facilities

Colliding beam devices have an overriding advantage in reaching very high interaction energies. This results from the fact that in a colliding beam machine all of the energy of the two beams can go into the interaction, whereas in a fixed-target accelerator most of the energy of the beam goes into forward momentum of the collision products. The successes of the ISR at CERN and of SPEAR at SLAC have established the technical feasibility and the scientific desirability of both building and doing research with such machines.

Colliding positron and electron beams produce matter-antimatter annihilations into pure energy and thus provide the source of an initial state of particular simplicity uncomplicated by strong interactions. Such a state of pure energy may then materialize into new particles. Furthermore, the electromagnetic force describing the electron-positron interaction is unique among the fundamental forces in that an extraordinarily accurate theoretical apparatus exists for its understanding. For these reasons, the electron-positron colliding beam facility is an ideal tool to look for and investigate the properties of new particles, as the recent discoveries at SPEAR attest. The distribution and yields of hadrons produced in electron-positron collisions give essential information on the character of the internal constituents of hadrons. Studies of electron-positron scattering and muon pair production in electron-positron collisions incisively test the validity of quantum electrodynamics as a theory of high energy electromagnetic interactions. Furthermore, at reaction or center-of-mass energies of about 30 GeV, the weak interactions are expected to become clearly observable. Using electron-positron annihilation in this energy domain, it will be possible to probe the workings of the weak force in a completely new setting other than in decays of particles or in neutrino or electron/muon interactions. Experience has demonstrated the scientific risk involved in bypassing any energy region. Therefore, it is important that the capabilities of any new electron-positron facility extend not only to energies as high as 15 GeV, but also as low as 4.5 GeV, the maximum energy covered by SPEAR and DORIS.

Proton-proton colliding beams provide reaction energies in a range which would be prohibitively expensive if obtained by any other means. The basic characteristics of the strong interaction can be studied at the highest attainable energies with this tool. It is important, for example, to know whether the rising total cross sections observed at Serpukhov, ISR, and FERMILAB, continue to grow with energy. Further, the strength of the weak force increases with higher energies and is anticipated to become comparable to that of the strong interactions at large angles and at reaction energies above a few hundred GeV. Therefore, proton-proton colliding beams in the hundred GeV range promise to be an effective way of observing new manifestations of weak interaction effects in conjunction with new strong interaction phenomena. Since massive particles, such as the as yet undiscovered carriers of the weak interaction, can only be produced at energies above a minimum threshold, a search for such particles requires a device capable of producing the highest energies. Such a machine provides the opportunity to extend the search for particles such as free quarks and magnetic monopoles, as well. It is also important that any new high energy proton-proton colliding beam facility be capable of operation at energies as low as 30 GeV, to permit coverage of the entire energy range above the ISR maximum.

In return for the giant leap in energy obtained by proton-proton colliding beams, however, a price is paid. Secondary beams cannot be produced, and the variety of interactions that can be studied by this means is restricted to those between the two beams of accelerated protons (although with major modifications, electron-proton, deuteron-proton, and antiproton-proton beam-beam collisions could be achieved). A fixed-target accelerator, while not attaining as high an effective reaction energy as a colliding beam machine, has the great virtue of a much larger interaction rate since the fixed-target consists of dense matter rather than being a diffuse beam of counter-rotating particles. Intense and well collimated secondary beams of various particle types otherwise unobtainable can be produced and used to study the interactions of π-mesons, K-mesons, neutrons, etc. with matter. It is also possible to produce high intensity neutrino, electron, muon, and photon beams, which are unique probes for studying weak and electromagnetic interactions and which also probe the internal constituents of matter.

On the basis of the discoveries of the past ten years, and with added emphasis from the very recent discoveries on substructure effects--new states of matter aas well as completely new kinds of phenomena--there is every indication of a potential breakthrough in understanding in each of the indicated research directions. The increasing interdependence of discovered phenomena is strongly indicative that the high energy physics research programs outlined by these future research directions will be remarkably productive.

D. Utilization of Existing Facilities

Whenever considering new facility needs within the framework of constrained funding, there is always a conflict with the need for efficient utilization of existing facilities. In recent years concern has increased about the utilization levels of the high energy accelerators, which are currently operating at levels of about 50 per cent. (The lower utilization results from decreased effective support levels at each established laboratory, which are the result of inflationary effects and a major new laboratory coming into operation. See Figure 2.)

An attempt has been made to find a single index which represents the utilization level of an accelerator facility. This requires a weighted function of the parameters of the accelerator such as operating energy and pulse rate, the number of simultaneous experiments, and the number of shifts operated. The utilization level is expressed as a percentage of a full program operating for 7500 hours per year and using the experimental facilities at the level which would be scientifically desirable and appropriate if the laboratory was fully responsive to the needs of users.

ERDA HIGH ENERGY PHYSICS LABORATORIES OPERATING COSTS

(FY 1976 DOLLARS)

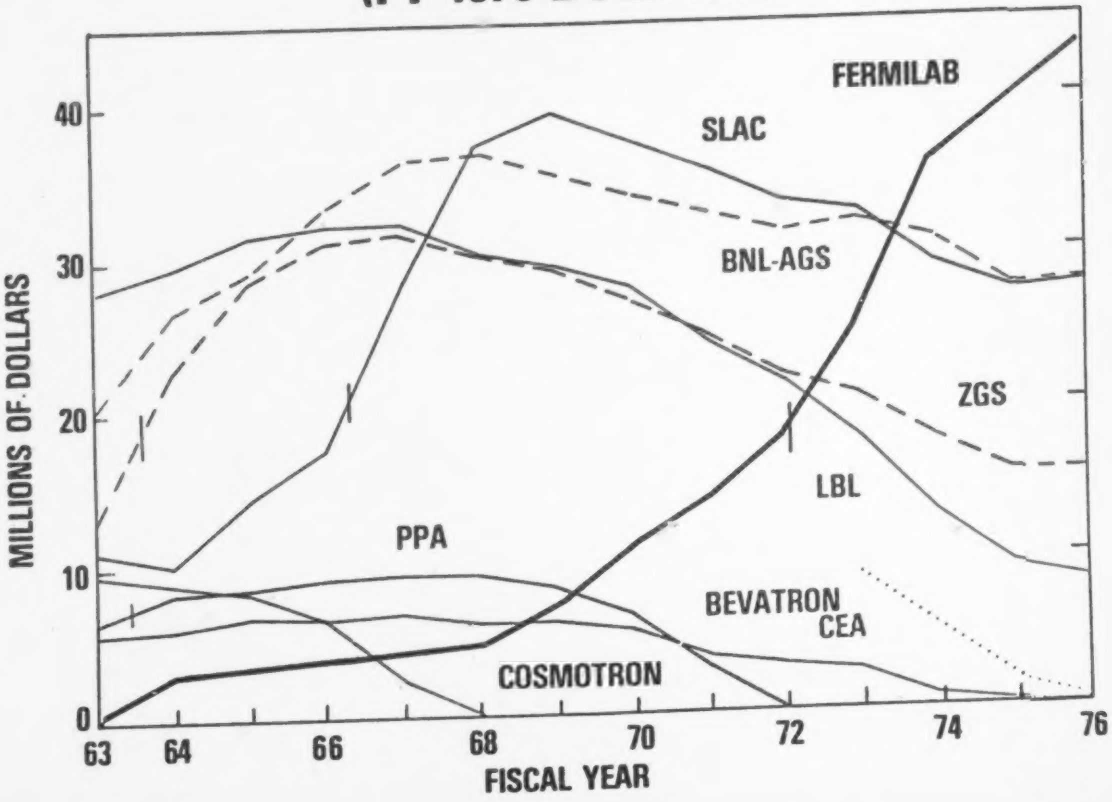


Figure 2

70 to 80 per cent level of utilization is considered a reasonable goal. Below that level, the efficiency and cost-effectiveness of the accelerator operation begins to decrease rapidly because of the large fixed costs associated with the existence of the facility. Utilization levels in the 95 per cent range would require very quick response to malfunction and would add significantly to manpower costs. This would be less than optimal cost-effectiveness, just as the present 50 per cent utilization is less than optimum. The 70 to 80 per cent range is a good compromise within the assumed budget constraints, between cost-effective operation, and a well-balanced program of productivity, innovation, and breadth of experimental capability.

During recent years, changes in the mode of operation of the accelerator facilities have been made in an attempt to maintain research output despite declining levels of scheduled operation. These include an emphasis on running several experiments simultaneously and improvements in accelerator intensity, flexibility, and reliability. This has significantly reduced the impact of decreased effective support on the overall output. The JCAE recognized this in its Authorization Report for FY 1975 which stated, "The AEC is commended, however, for its efforts in the use of simultaneous experiments to maximize the scientific results obtainable from the reduced operations." This effort to maximize scientific output will, of course, continue.

It has been generally assumed that if a facility is to be shut down, the logical candidate would be the ZGS. This was extensively studied in 1974 by an AEC/NSF ZGS Study Committee and two Subpanels. Their conclusion was that this is a most inopportune time for a shutdown of the ZGS, in that it currently has a very vigorous and innovative physics program with unique capabilities serving experimental groups from more than 20 universities. Polarization effects on the strongly interacting particles can be studied at the ZGS in ways unavailable at any other accelerator. These are large effects bearing on the relationships between the electromagnetic and strong interactions. Several aspects of the ZGS program, such as the neutrino experiments and the polarization studies, have just begun. The Committee concluded that at least four to five years are needed to complete the ZGS program which clearly can be seen as important and unique.

In developing the desired 1985 facility posture as well as each of the alternate plans discussed in Section V, care has been given to achieving a reasonable balance between utilization of existing facilities and the provision of new facilities and their utilization.

IV. Long-Term Facility Posture

A. Background

The development of the long-term (1985) facility posture for high energy physics outlined below, and the priorities and long-range plans subsequently discussed, have drawn heavily on the deliberations of the two Subpanels on New Facilities of the High Energy Physics Advisory Panel, which met in 1974 and in 1975, and on an extensive history of considerations and discussions with HEPAP. Although the charges to the Subpanels each focused on a single fiscal year, as did their recommendations, their deliberations and recommendations were necessarily made within a long-range, overall context for the national program. The Subpanel reports are included as Appendices B and C of this report.

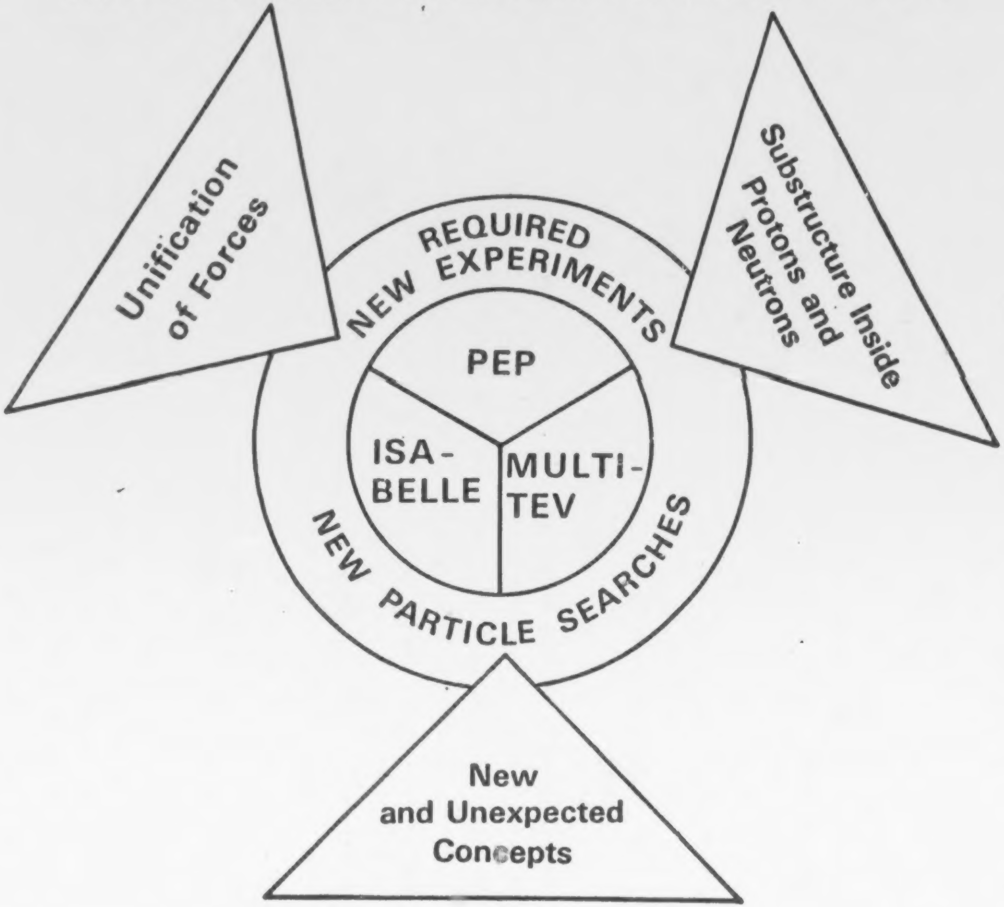
The posture developed here assumed the funding constraints provided for guidance to the 1974 Subpanel on New Facilities, which correspond to Level 1 in the OMB request for this report. These funding constraints represent an annual level of effort for the national program somewhat below the overall level of effort which existed in the past FY 1965 to FY 1974 ten-year period, and below the level represented by the guidelines of the current ERDA five-year planning projections for High Energy Physics. The impact of the alternative funding constraint levels is discussed in a subsequent section of the report.

The field of high energy physics is one of the most exciting fields of basic research in the world today and is in a period of high activity and promise. New exciting results, new ideas, and new methods are appearing on many fronts. The 1975 Subpanel noted that in all of the major laboratories, whether one works with proton, electron, photon, or neutrino beams, there are indications of potential breakthroughs wherever one looks. They pointed out the remarks of the 1974 Subpanel relating to the enthusiasm generated by the prospect of major breakthroughs at that time, and noted that the beginnings of some of those breakthroughs had appeared in the intervening year, giving added impetus and urgency to the need for construction of new facilities to open up higher energy regions for exploration. Pointing out the increasing interdependence of the research done with different types of particle probes, both Subpanels emphasized the importance of pushing forward to higher energies with electron-positron colliding beams, with proton-proton colliding beams, and with proton beams colliding with fixed targets. The facility posture outlined here is based on this three-pronged approach to higher energy which was unanimously recommended by the two Subpanels and by HEPAP. The need for the three higher energy facilities ~~is~~ indicated in Figure 3 as following from the need for higher energy experiments to pursue the three main areas of investigation of Figure 1.

In attempting to develop long-range plans for High Energy Physics, it must be recognized that it is difficult, and probably impossible to blueprint very far in advance the detailed course of a basic research program. Progress in basic research inherently follows a course which evolves and is continually being modified by the impact of the most recent developments. The plans developed in this report would provide for the high priority needs of the High Energy Physics Program as seen now, in the light of recent discoveries, the present research status, and current technology. New scientific and technical knowledge will undoubtedly dictate, at a later date, modifications of these plans and the posture presented in this report.

Nevertheless, long-range planning and a determination of the Government's position with respect to long range support of High Energy Physics are required and essential factors for its effective progress. This is true because of the large and expensive facilities which are required for research in the science and the long time scales which are its inherent features--construction of a major new facility typically takes eight to ten years from proposal to productive use; an experiment typically takes

IMPACT OF RECENT DISCOVERIES ON FACILITY PLANS


Unification of Forces
Substructure Inside Protons and Neutrons
NEW REQUIRED EXPERIMENTS
PEP
ISA-BELLE
MULTI-TEV
NEW PARTICLE SEARCHES
New and Unexpected Concepts

three years from proposal to experimental results and publication. This long-range nature of the science requires long-range planning and a stability of support, while its evolutionary nature requires a concurrent flexibility in the long range plans.

B. Considerations and Assumptions

The considerations and assumptions used in developing the long-term facility posture outlined for high energy physics in this report include the following:

1. High Energy Physics is a forefront fundamental science in which it is important that the U.S. maintain a leading role.

2. It is essential for a science at the frontiers of the unknown to devote a substantial part of the program effort to the provision of new and innovative facilities. Past experience in high energy physics shows that an average expenditure of 20 to 25 per cent of the total program funding for construction of such facilities provides a healthy balance between exploitation of research opportunities at existing facilities and development of new capabilities for future frontier research needs.

3. The interdependence of the research done with different particle probes mandates a three-pronged approach to higher energies via positron-electron colliding beams, proton-proton colliding beams, and protons colliding with fixed targets, for maintaining a vital and productive High Energy Physics Program for the period of the 1980's.

4. Firm funding constraints will remain on the program. It is assumed that over the projected FY 1976 to FY 1985 ten-year period these constraints will permit an average annual level of effort for the national program (ERDA and NSF) which in operating and equipment will be about the same as it was for the past FY 1965 to

FY 1974 ten-year period (about $245 million per year in FY 1976 dollars); and which in construction effort for new facilities will be comparable to about two-thirds of the annual average over the past FY 1960 to FY 1974 fifteen-year period (about $50 million per year in FY 1976 dollars).

5. This assumption implies that U.S. funding for high energy physics will continue to substantially trail that for the Western European program which is currently about two and one-half times greater than the U.S. (The total Western European funding for high energy physics is about twice that for the European CERN laboratory which is funded in 1975 at 645 million Swiss francs, or about $238 million, using the current rate of exchange. The total U.S. funding for high energy physics (ERDA and NSF) is about $190 million in the current FY 1976 request to Congress.)

6. In providing new physics capabilities, major advantage can be taken of the outstanding assets of the High Energy Physics Program which have been built up over previous years. These assets include the major accelerators and support facilities and buildings, which are in excellent and forefront condition, and include an outstanding cadre of highly skilled scientific and technical manpower with a proven record of success and leadership.

7. High energy physics is a particularly fruitful field for international collaboration.* Large accelerators are available in the U.S., Western Europe, and the USSR; and the scientific results are freely available. Cooperation and coordination of research between the various areas have been substantial and successful, including the use of unique facilities in one area by scientists from the other areas, and the participation of Western European physicists in the two recent HEPAP Subpanels on New Facilities. The continuation and enhancement of international cooperation and collaborations are important for U.S. progress and success in this field and need to be vigorously pursued.

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*See Appendix D--International Aspects of High Energy Physics--Page 123

It is conceived that the next step in fixed-target machines beyond the multi-TeV machine projected here for FERMILAB would require a cooperative international effort. An international study group currently is being set up to look at the potential problems that would be involved in such a major technological and scientific collaborative effort. Such a major project is probably beyond the ten years of this projection.

8. The high cost of research in high energy physics exerts a strong and indeed controlling influence on the facility posture of the field. The total number of facilities has shrunk as their high capital and operating costs have forced increased centralization. The high cost dictates making major use of existing facilities and other program assets to develop the required new facilities discussed below. Many unique capabilities desirable for progress of the field would be met only in foreign laboratories. In particular, this plan assumes that scientific redundancy which is important for verification purposes will largely be possible only on an international basis.

C. 1985 Facility Posture

Although U.S. leadership in high energy physics is no longer unchallenged, it is believed that the long-term facility posture outlined below will maintain a strong and leading role for the U.S. in this forefront science. This includes recognition that the assumed funding constraints would limit the average annual support level for the U.S. program to about two-thirds that of the European program and about equivalent to the USSR program. The primary reason for the optimism is the belief that the projected facility posture is a good one, made possible by: (1) the major use of available capital and manpower assets of the current program in developing the new capabilities; (2) the excellent condition and forefront nature of the existing facilities; and (3) the high caliber and enthusiasm of the personnel. In addition, the U.S. program benefits significantly from the diversity of several major laboratories, in contrast to the European program which is heavily focused at one laboratory, CERN.

The 1985 facilities posture for the U.S. High Energy Physics Program, under the assumptions mentioned above, would include:

1. Facility utilization levels of about 70 to 75 per cent.

2. An 15 GeV x 15 GeV positron-electron colliding beams facility (PEP) jointly operated by SLAC and LBL, using the present SLAC linac as an injector. (SPEAR would be shut down when PEP comes into operation.)

3. Higher energy electron (and positron) beams on fixed-targets at the SLAC linear accelerator as a result of improvements already underway (SLED). In 1985 the SLAC linac would be operating primarily in this higher energy (20-45 GeV) region for fixed target physics.

4. A 200 GeV x 200 GeV proton-proton colliding beams facility (ISABELLE) in operation at BNL using the AGS as an injector. With authorization in FY 1978, initial operation would be about 1983.

5. An AGS proton beam, fixed-target program, up to 33 GeV, with broadened emphasis to include the lower energies, compensating for the ZGS which might be shutdown about FY 1981.

6. A FERMILAB facility which would have had substantial growth in the experimental areas and in its capabilities. Some high energy proton beam, fixed-target capability up to perhaps 800-1000 GeV would be available based on the successful development of the Energy Saver/Doubler. This development would be an important expansion of the FERMILAB capability, hopefully having been successfully accomplished shortly after the operation of the 400 GeV SPS at CERN enters its second, heavily equipped stage in 1978.

7. A multi-TeV proton fixed-target accelerator and/or colliding beams facility would be nearing completion of construction at FERMILAB. It would make use of the superconducting magnet and cryogenic developments of the Energy Saver/Doubler program and would use the FERMILAB machine together with the Energy Saver/Doubler as an injector. Construction would have started about 1980 and be scheduled for completion about 1986.

V. Plans and Priorities

The long-range plans and priorities for high energy physics outlined here are formulated to exploit existing facilities during their productive lifetimes while making an efficient transition from the present to the future facility posture. The plans are designed within a constrained funding total to maintain the vigor and productivity of the U.S. High Energy Physics Program, and to maintain a competitive world posture.

The long term facility posture would give high priority to an improved utilization of the facilities from the present approximately 50 per cent level to a 70 to 75 per cent level in the first few years of the ten year plan. The interdependence of research with the electron-positron colliding beams, the proton-proton colliding beams, and the fixed target proton machines, mandates movement to higher energy on all three fronts. Timewise, however, the first priority for new facility construction is the positron-electron project, followed by the proton-proton colliding beams device.

A. Construction of New Facilities

Two of the three new facilities which should be a central part of the facility posture for 1985 have been formally proposed as additions to existing laboratories. The University of California and Stanford University have proposed the joint construction by SLAC and LBL of a 15 GeV x 15 GeV positron-electron colliding beam device (PEP) which would use the existing linear accelerator at SLAC as an injector. It would also use existing SLAC support facilities as well as SLAC and LBL scientific and technical manpower. This project uses well-established technology, and construction can begin as soon as funds are available. Based on Congressional action to date, construction of PEP is planned to begin in FY 1976, with initial operation for research in FY 1980.

Brookhaven National Laboratory has proposed the construction of a 200 GeV x 200 GeV proton-proton colliding beam facility (ISABELLE which would use the AGS as an injector. This facility could operate at all beam energies between 30 GeV (the maximum of the CERN ISR) and 200 GeV, a colliding beam energy equivalent to that from a fixed-target machine of 85,000 GeV. The project would benefit from the availability of experienced BNL scientific and technical personnel and would use available support facilities. Research and development on superconducting magnets, beam dynamics, cryogenic systems, and other systems for a proton-proton colliding beam system has been increased at BNL for FY 1976. Although the 1975 Subpanel on New Facilities recommended that construction of ISABELLE begin in FY 1977, the plan outlined here would delay construction until FY 1978, with initial operation for research in FY 1983. This facility would give the United States its first capability in proton-proton colliding beam physics, where the U.S. is presently entirely dependent on the Western European effort.

The third major new component of the 1985 facility posture is a multi-TeV fixed-target proton accelerator with beam energies of at least 2 TeV, and perhaps as high as 5 TeV. FERMILAB, with its existing 400-500 GeV accelerator, adequate available space, and strong experimental and support facilities, is an obvious location. This facility would rely heavily on the technology developed in the Energy Saver/Doubler research and development program and other superconducting accelerator development programs, and would use the FERMILAB accelerator as an injector. Construction of this facility could begin in FY 1980 and would then be available as a research tool shortly after 1985.

The long-range plan for construction includes accelerator improvement projects (AIP) at a level of about $8 million per year. These funds permit construction of relatively small scale additions and modifications to the existing accelerator facilities. Some elements aim at increasing the reliability and efficiency of accelerator operations while others are geared to providing improved or modified capabilities required by specific experimental needs. Examples of AIP efforts include intensity improvements and new extraction techniques, increasing the energy of

SLAC to about 45 GeV by using the SLED concept, modification of the target areas at FERMILAB to accommodate beams of higher energy and intensity, and upgrading of computer control systems to improve operating efficiency and reduce electric power consumption. The need for AIP funding is particularly important at FERMILAB in the next few years, where many of the beam switchyard and experimental facilities are matched to its original energy of 200 GeV which has now been far surpassed.

The importance of accelerator improvement projects cannot be overemphasized. For a small fraction of the capital investment at the various accelerators, accelerator improvements have increased the overall efficiency, effectiveness, and productivity many times. They have permitted upgrading of the facilities to avoid the obsolescence which otherwise quickly overtakes a research device and to accommodate forefront experimental techniques and needs.

Figure 4 shows the history of construction funding for the program since FY 1960, and the projected funding required for the next decade to implement the program described above.

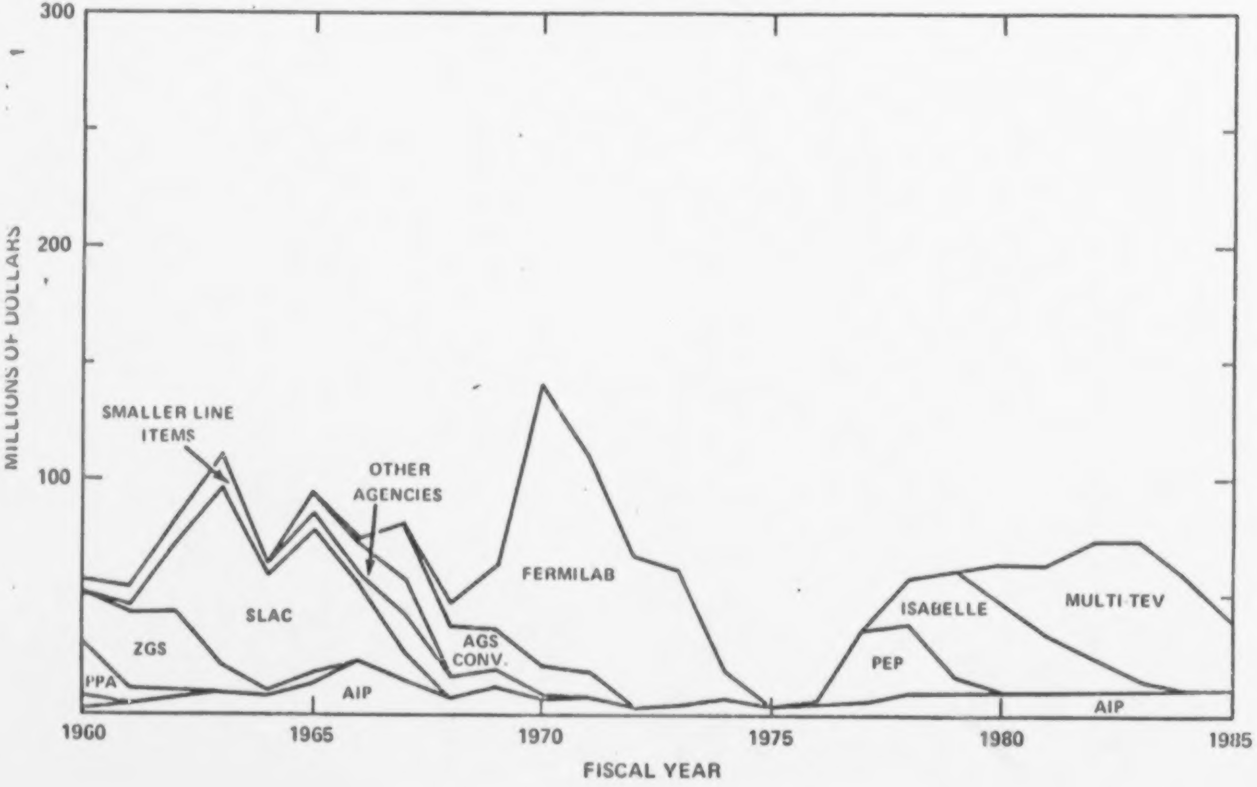
B. Operations of Facilities

During the next few years, the utilization levels of the AGS, FERMILAB, SLAC, and the ZGS would be increased from the present levels of about 50 per cent to about 70 to 75 per cent. This overall utilization level would then be maintained, although there would be a continuing evolution in the character of the program resulting from a phasing out of some existing efforts and phasing in of others, as the physics advances and as new capabilities become available.

Operation of SPEAR at SLAC would continue with high priority until PEP begins operation. Shortly after this time SPEAR would be closed for high energy physics use and the research effort transferred to PEP. The incremental cost of operating PEP would be minimized further by the use of available SLAC support facilities. Major expenditures for the

Figure 4

CONSTRUCTION FUNDING PLAN
FOR U.S. HIGH ENERGY PHYSICS PROGRAM
(IN FY 1976 DOLLARS)



(9/24/75)

initial complement of PEP equipment would occur in FY 1978 and FY 1979 to be ready for initial experiments in FY 1980. The scope of the fixed-target program at SLAC would shift in about FY 1980 to emphasize the new range of energies (up to about 45 GeV) provided by the SLED program.

Higher levels of utilization would permit increased use of the unique capabilities of the ZGS over the next few years. Exploitation of these capabilities might then be sufficiently completed to permit phasing out operation of the ZGS as ISABELLE is phased into the program. Some of the facilities of the ZGS complex, including shop and electronics, would be converted to user group support. The high energy physics personnel retained at ANL would devote their efforts to research as users at other accelerators and to apparatus and device development.

The fixed-target program at the AGS would retain its present character until the ZGS phase out, at which time it would be augmented to cover the 3 to 12 GeV energy range now covered by the ZGS. Other parts of the current fixed-target program at the AGS would be phased down, however, so that the overall level of the fixed-target program at the AGS would remain about constant. By 1985 the principal focus of the high energy physics effort at BNL would be on ISABELLE. Major equipment expenditures for initial ISABELLE experiments are projected for FY 1981 and FY 1982.

The next few years will see an increasingly active research program at FERMILAB at energies of 400 to 500 GeV and fuller development of the FERMILAB capabilities. The Energy Saver/Doubler, which is presently under development, should be in successful operation by 1979-1980. It would permit major electric power savings at FERMILAB, and would also provide some capability for research at energies as high as 800 to 1000 GeV. The higher energies may be essential to the competitiveness of the U.S. High Energy Physics Program when the CERN 400 GeV SPS in 1978 enters its second phase of operation with the exceptionally well instrumented North Experimental Area. Since the multi-TeV addition at FERMILAB would not be completed before FY 1986, only pre-operating funds are required for it within the time period under consideration.

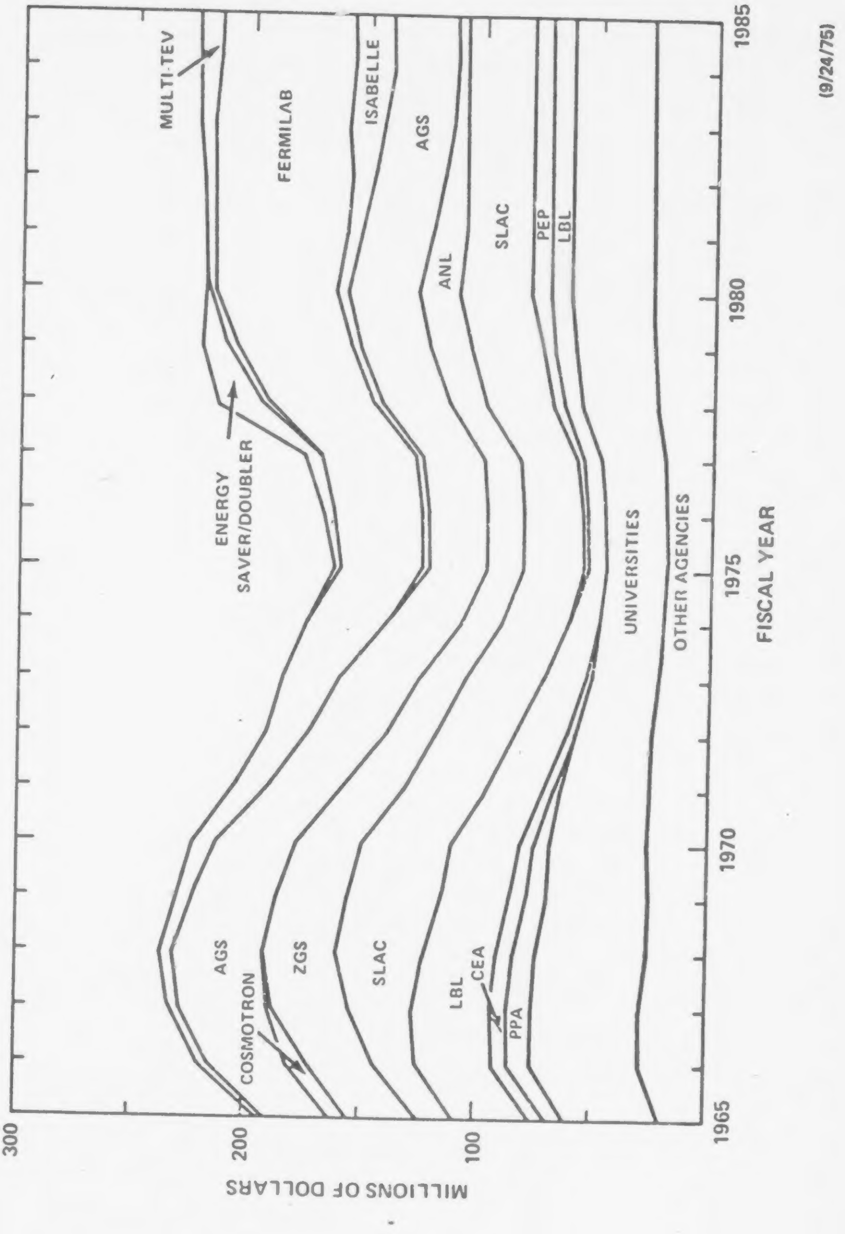
By 1985, under the assumptions used here, FERMILAB, AGS, and SLAC would be the only major accelerator centers for high energy physics in this country. Each, however, would have a program which would be on the forefront of the field, unique, and complementary to the program at the other two. The support required for user groups, mostly from universities, who plan and execute the experiments on these accelerator facilities, is assumed to remain proportional to the facility operating expenses.

Figure 5 shows the projected distribution of operating funds through FY 1985 for the plan.

An important element related to the effective operation of facilities is the capital equipment funding which is used to provide the apparatus needed to carry out the experiments by which new discoveries are made. In the years immediately preceding operation of a new facility, as well as in the first few years of operation, equipment needs are high since an initial complement of large general purpose items, such as spectrometers and bubble chambers, must be provided for long term use by the laboratory. Afterwards, equipment needs fall primarily into two categories: (1) general purpose equipment which broadens the opportunities available to experimenters at a given laboratory; and, (2) equipment required to carry out specific experiments accepted for scheduling by a laboratory. Both types of equipment items for ongoing facilities are essential to the efficient utilization of the accelerator. Included among capital equipment needs are the large central computer systems and the smaller computers which are on-line with many experiments. High Energy Physics research depends heavily on large computer systems to efficiently process the large volume of information generated by experiments and for

Figure 5

OPERATING FUNDING PLAN
U.S. HIGH ENERGY PHYSICS PROGRAM
(IN FY 1976 DOLLARS)



theoretical analyses and models. Today, on-line computer control and monitoring of experiments and accelerators are required to achieve greatest efficiency. The national need to conserve electrical energy has at this time created an additional special need for capital equipment items which are designed to decrease the electric power needs of the experimental facilities.

There is no simple formula to determine the optimum ratio of equipment funding to construction cost for a new facility and/or the ratio of equipment funding to operations funding for the program as a whole. Historically, a level of 20 to 30 per cent of construction costs has been required for the initial complement of equipment for a new facility. The ratio of equipment funds to operating funds for established laboratories was also once in the 20 to 30 per cent range but is now about 10 per cent. The overall program average for the facility posture developed here is about 15 per cent of the level of operating funds.

The long range equipment funding levels which are shown in Figure 6 were determined by considering the needs for continuing the programs at each of the existing facilities, the incremental needs for the initial complement of equipment at new facilities, and the continuing needs at new facilities. Figure 7 shows the total program funding.

Figure 6

EQUIPMENT FUNDING PLAN
U.S. HIGH ENERGY PHYSICS PROGRAM
(IN FY 1976 DOLLARS)

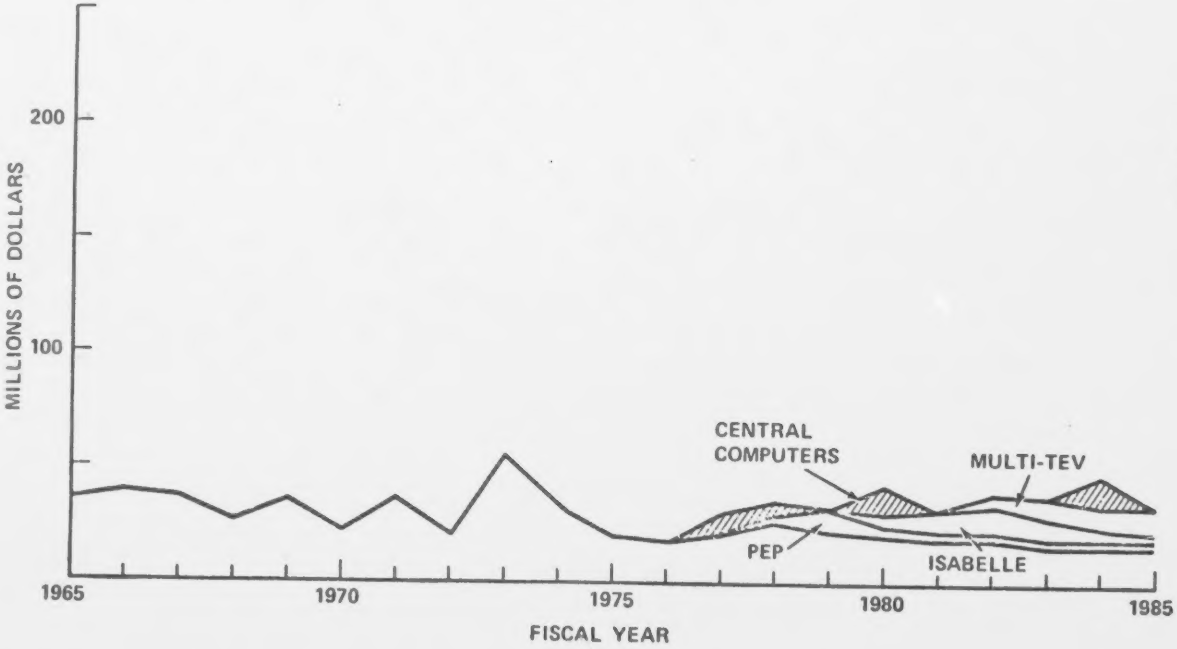
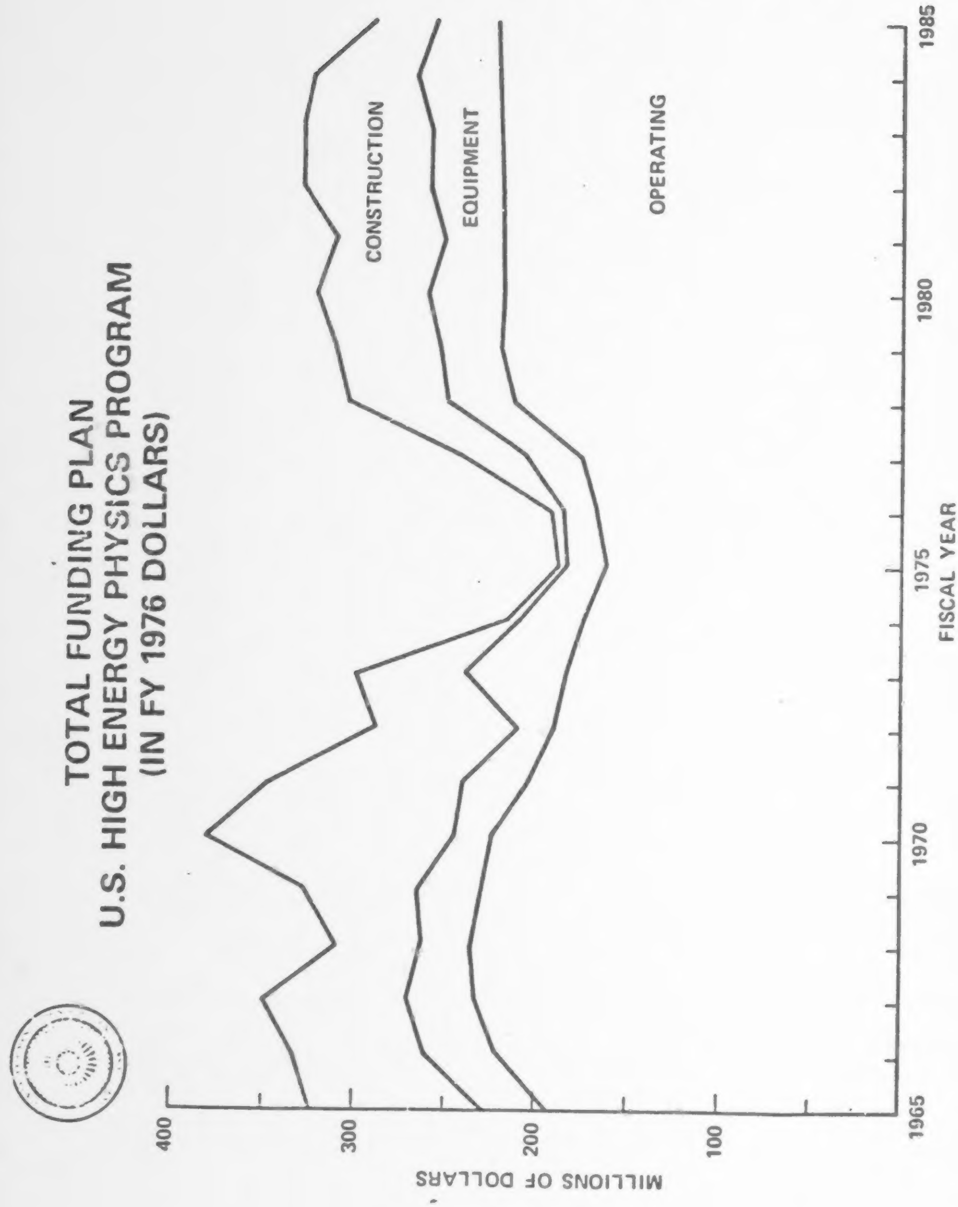


Figure 7



TOTAL FUNDING PLAN
U.S. HIGH ENERGY PHYSICS PROGRAM
(IN FY 1976 DOLLARS)

VI. Alternative Funding Levels

A. Definition of Levels

Consideration of the effects of several funding constraint levels were agreed to in discussions between ERDA and OMB. These alternative levels and their impacts are discussed below. In each case the funding constraints refer to the average annual level of effort over the ten year period from FY 1976 through FY 1985 and include total support for the program from all Federal sources. All funds are expressed in FY 1976 dollars of constant purchasing power.

Four funding levels have been considered:

Level I

1. Operating and Equipment. Average annual level of effort same as for FY 1965 to FY 1974. (About $245 million per year.)
2. New Facility Construction. Average annual level of effort about two-thirds that for FY 1960 to FY 1974. (About $50 million per year.)

Level II is a level close to optimum for the scientific opportunities of the program.

Level III

1. Operating and Equipment. Average annual level same as represented by FY 1976 level of funding. (About $190 million per year.)
2. New Facility Construction. Average annual level is $20 million per year.

Level IV is about midway between Levels I and III.

B. Analysis of Impact

1. Level I

The 1985 facility posture and long range plans presented in Section IV and V of this report were structured to meet the constraints of this level. Level I is below the average level at which the U.S. program operated in the FY 1965 through FY 1974 period. It is also substantially less than funding for the European high energy physics program. Nevertheless, the program projected for Level I is considered a good program which would maintain a strong and leading role for the U.S. It represents a healthy mixture of effective utilization of operating facilities and timely provision of new innovative facilities.

2. Level II

The U.S. High Energy Physics Program has a long record of excellence and success of which this past year's discoveries are the latest examples. A large number of fundamental discoveries have come from the program and the possibility for new discoveries and breakthroughs has seldom been as great as it is today. A budget level which would permit an optimum utilization of the existing forefront capabilities and permit the construction of new facilities as they are needed and can be efficiently exploited is well justified.

Included within such a program would be construction of the proposed Cornell 8 GeV electron-positron colliding beam facility to permit a more rapid and comprehensive study of the 4 to 8 GeV energy region in the important area of electron-positron colliding beam physics. Also envisaged in such a program would be the addition of other colliding beam capabilities at PEP or ISABELLE. These would include proton-antiproton and proton-deuteron capabilities onto the proton-proton rings and an electron-proton capability onto either the proton-proton or the electron-positron rings. The addition of a superconducting stretcher ring at the AGS, and possibly the ZGS, would have the multiple advantages of saving electric power, increasing the duty factor by as much as a factor of five and doubling the intensity, for a relatively low construction cost and no overall increase in operating costs. Enhanced Accelerator Improvement Projects funding would pay substantial dividends by increasing the capability and operating efficiency of existing facilities. Such a budget would also include funds beyond the range of Level I for new beam lines, new and improved experimental areas, and large detection systems to be used with the existing as well as the new accelerators.

This level would give greater priority to development efforts in the program, both for improving accelerator capabilities and efficiencies and for instrumentation and detection techniques and capabilities, activities which have fallen to rather low levels in recent years. In particular, a more rapid development of the FERMILAB Energy Saver/Doubler promises to reduce overall costs both for that project itself, and for operation of the entire FERMILAB.

A high priority would be to increase the utilization of the new facilities as well as productive ongoing programs from the 70 to 75 per cent range of Level I to about an 85 per cent level, which is near optimum. This is an increase of about 20 per cent in utilization but would require only about a 7 to 10 per cent funding increase, since there is about a two to three leverage factor ratio of utilization over funding at these levels.

Due to the complexity of assessing the full scientific potential of high energy physics, ascertaining the optimum program mix, and estimating reasonable funding and manpower needs over the next ten to fifteen years, we have not attempted here to fix either the cost or the complete structure of the program which fits best the definition of this level. A Subpanel of HEPAP has been formed to study this matter in detail and it will submit its report in six to eight months.

3. Level III

To understand the full impact of this budget level on the program, it is helpful to compare it with past funding levels; $245 million per year average for Operating and Equipment in the FY 1965 to FY 1974 base period, and $74 million per year average for Construction in the FY 1960 to FY 1974 base period. Thus, this level limits Operating and Equipment to less than 80 per cent, and Construction to less than 30 per cent of the average level of the respective base periods.

The need for new facilities is essential and the urgency for a construction start on PEP remains foremost. Neither of the remaining two top priority construction projects can be completed within the constraints of this level. One could be initiated during the projected ten year period, but there would be insufficient funds to complete it until late in the 1980's. The need for both a high energy proton-proton colliding beams facility and a high energy fixed target accelerator would remain. Although present thinking would indicate proceeding with proton-proton colliding beams as the next large construction project after PEP, the initiation of the second project is so delayed under the constraints of this level that one is unable to conjecture which should come first. The decision on which it should be, should be postponed. The 1975 New Facilities Subpanel recommended that in this situation significant investments be made in accelerator development and superconducting technology at both BNL and FERMILAB, notwithstanding the severe limitations set by the overall funding level.

The impact of this level on both the long term and short term aspects of the program is surprisingly severe. Utilization of all laboratories would continue at no more than the present 50 per cent level. Use of new capabilities, some of which are still coming on at FERMILAB, would have to be funded by reductions elsewhere. Indeed, if the Energy Saver/Doubler project is to continue at adequate levels during the next few years, it would require substantial reductions in the utilization levels during those years; to the low 40 per cent range. Although this level of utilization may be considered almost irresponsible, the competing Energy Saver/Doubler also has a very high program priority. Its successful development offers major savings in electric power consumption and operating costs for FERMILAB, as well as significant improvements in accelerator energy and duty cycle. It also represents a technology development effort of substantial potential benefit to a future higher energy accelerator. Even with the 40 per cent range of utilization levels, the Energy Saver/Doubler effort would be slowed, with consequent increase in its overall cost and delay in receiving its benefits.

All in all, this budget level would be damaging to the U.S. effort in high energy physics. The U.S. program, which only a few years ago was unexcelled in the world, and which now shares a number of firsts in important discoveries with the European laboratories, would clearly lose its world leadership role at this level. The future of high energy physics in the U.S. would be placed in question, and the advancement of the science world-wide would be significantly slowed.

4. Level IV

This level was thoroughly examined by the 1975 Subpanel on New Facilities. They initially considered the implications of constraints at this level as exactly midway between the various constraints of Levels I and III; i.e., $215 million for Operating and Equipment, and $35 million for Construction. However, they noted that:

> "There has been no major facility construction authorized for high energy physics since 1968. Since that time, important developments in physics and accelerator technology have opened three major paths for high energy physics to follow: electron storage rings, proton storage rings, and fixed target accelerators. We cannot afford scientifically to neglect or appreciably de-emphasize any one of them. We view this three-pronged approach to the future as of primary importance to the well-being of high energy physics in the U.S. in the next decade."

They then recommended that at least $50 million should be allocated to construction even within the limited budget of this level. Recognizing that this would require significant sacrifices within the present program, including no recovery from the present levels of utilization they nonetheless considered these sacrifices worth making to insure the future of the field.

The ERDA High Energy Physics Program Staff made an analysis of the impact on accelerator utilization of a $200 million level for all operating and equipment, and concluded that it would be unacceptably low. While recognizing the critical importance of new construction for the program, they felt that the operating and equipment level would have to be increased above $200 million. The impact of Level IV, therefore, is discussed below on the basis of an average annual level of $210 million for Operating and Equipment, and an average annual level of $40 million for Construction.

Under these assumptions, the three-pronged approach so strongly recommended by the two Subpanels and by HEPAP can be pursued, but with a large stretch-out and delay in the completion of the Multi-TeV accelerator. The construction of PEP would begin in FY 1976 and construction of a proton-proton colliding beam facility would be initiated in FY 1978. The Multi-TeV accelerator construction start, however, would be delayed until FY 1983, and could not be completed until about 1989--well beyond the period of this study. As under Level I, all of the new capabilities would be located and designed to use existing facilities and all three new machines would use superconducting technology, wherever feasible, to reduce both electric power needs and operating costs. AIP funding for the program would be held to about $7 million per year to provide the most important improvements and upgradings required for the operating accelerator and colliding beam facilities.

Within the operating and equipment constraints of this level, high priority would be given to expeditious development of the Energy Saver/Doubler project at FERMILAB in view of the combination of reduced energy consumption and new physics capabilities provided. However, other operating and equipment needs would suffer substantially relative to Level I. The incremental funding for initial complements of equipment for the new capabilities would be substantially reduced restricting the scope of the research program at these facilities. Operation of the capabilities even near current restricted utilization levels will require elimination of some substantial components of ongoing programs. Little or no improvement of the current low utilization levels of the operating facilities will be possible.

VII. Long Range Planning and Budgeting

The need for long range planning is indicated by the natural time scales associated with the activity being planned. Research in high energy physics inherently involves long time scales. Construction of a large particle accelerator requires about a decade from the generation of the proposal to the time when the facility begins to make an impact on physics. The elapsed time from proposal to publication of typical high energy physics experiments is about three years. From design to use of a major piece of experimental equipment such as the fifteen-foot bubble chamber at FERMILAB, the LASS spectrometer at SLAC, or the Multi-Particle Spectrometer at BNL, takes three to five years. To become professionally productive in the field requires about six years on the average, which is roughly equivalent to the time spent in graduate study in physics.

The high costs of the large accelerators and associated facilities required for high energy physics permit only a small number of such facilities. The conflict between the small number of accelerators and the broad interest in participating in such fundamental research has led to the present high energy program structure with a few large national accelerator centers and a broad national participation of university user groups.

Furthermore, as has been outlined in previous sections of this report, it is essential to the nature of the science that there be a continual infusion of new innovative, technological capabilities. The cost factor again affects this in that only the high priority needs can be met.

All of the above features of high energy physics demand careful, long-term planning. This, of course, has long been recognized and there has been an extensive history of such planning on a national scale. Appendix E includes a listing of reports of past studies that have dealt with long range needs of the program.

To develop effective long range planning and budgeting procedures it is important also to remember the concurrent need for flexibility in the long-term plans. As discussed earlier in this report, the most productive directions for a basic research program are modified by the latest research discoveries, which illuminate both the significance of the past results and indicate new experimental and theoretical requirements. Thus, the emphasis of the research evolves with time and in a way that cannot be predicted very far in advance. Technological developments, of course, also determine the limits for advancement of technological capabilities.

In discussing the issue of long range planning and the inefficiencies and program damage that have resulted from the lack of agreed-on long range planning and budgeting for high energy physics, it is important to separate the long range budgeting issue from the absolute budget level of any one fiscal period. On the other hand, effects on the program of sudden budget changes are affected appreciably by the relative level of one fiscal period to that of the previous period. The difficulties of uncertain budgets are compounded and the effects of late changes much more severe on the program when the level of effective funding is decreasing. This, of course, results from the greater inflexibility of a program with a decreasing or constant budget to accommodate changes without damage to the ongoing program.

Substantial problems have arisen in the past from unexpected fluctuations in funding. Significant layoffs at SLAC and abrupt reductions in staff at the AGS, the installation of new improvements at the Princeton-Pennsylvania Accelerator shortly before it was closed, and unanticipated reductions in accelerator operations due to a midyear budget decrease are examples of damaging events which have occurred in recent years and which could have been avoided or reduced by longer range budgeting. Many problems have arisen in recent years because of unexpectedly severe inflation.

Long range budget planning would be especially helpful to the national laboratories in allocating the resources they can devote to the experiments that have been already approved. Faced with many proposals for worthwhile experiments, as they are today, the laboratories strive to accept and schedule as many as possible. Experiments must be scheduled many months in advance, and commitments made both by the experimenters and the laboratory make subsequent changes difficult to accommodate. Since most of the experimenters are university staff members, their university obligations and teaching schedules must be interleaved and coordinated with the laboratory operating schedule.

Unlike many operations for which materials constitute the major expense, it is not possible to switch on or off the flow of supplies to match the budget in high energy physics research. Much of the cost of the program is in salaries for exceptionally skilled manpower who cannot easily or quickly be replaced.

The time scales discussed earlier illustrate the need for both long range planning and program stability. The present system of annual budgets, whose level often is not determined until after the fiscal period has begun, clearly does not constitute a good match for these natural periods. The problem is compunded by there being no flexibility for operating funds to be overrun or carried over between fiscal years. This severely limits the ability of a laboratory to respond to budget changes or to late changes in the program made necessary by, for example, the malfunction of a piece of equipment or the injection of a new high priority experiment into the schedule. A very small carry over or overrun, perhaps as small as 2 per cent, either way, and corresponding to a week of operation, would significantly relieve this problem.

An example of the successful implementation of longer range budgeting is provided by the "Bannier Procedure" which is used at CERN. Firm budgets for a given fiscal year are approved one full year prior to its beginning, and provisional budgets are approved for an additional two years. Each of these budgets is in funds of constant purchasing power, and the actual funding is adjusted as necessary by changes in the price index. The system is flexible in that the funding may be modified by unanticipated economic or scientific developments; however, only major problems are allowed to influence the firm years. The procedure has served CERN and its member States well, and provides a reliable guide to future funding on which realistic long range plans can be based. A similar procedure would make a significant contribution to the effectiveness of the U.S. program.

A planning and budgeting system similar to the "Bannier Procedure" could be developed in the U.S., based on a "best efforts" commitment between the OMB and the agencies of the Federal Government which support high energy physics (ERDA and NSF). This would permit more meaningful longer range plans which could be followed in a consistent fashion. An example of how such a system might work is outlined in Appendix F.

The more effective planning and budgeting permitted by such a system would help avoid costly disruptions and discontinuities in the ongoing program caused by unanticipated budget changes. Ideally, such longer range budgeting and planning would permit the proper pacing of new facilities with their new technological capabilities, optimization of the use of capabilities of existing facilities, and phase-out of older facilities in an orderly fashion with proper exploitation of their scientifically valuable capabilities. The increased stability in the planning process would also permit young scientists to make better career decisions. Effective long-range budgeting would in all likelihood eliminate sharp changes in budget levels. A sharp decrease inevitably leads to a disproportionate reduction in program productivity, which is compounded if the decrease is unanticipated. Large increases should be coordinated with the inauguration of new facilities to get good use of such facilities.

The question on long range planning also asks, "How would the present yearly review process be improved by longer range plans based on alternative funding scenarios?" Consideration of alternative funding scenarios may be helpful in arriving at the most appropriate long range plan. However, the yearly review process would not be improved by longer range plans based on alternative funding scenarios. The picking of a new alternative level each year, for example, would be the antithesis of a long range plan.

The greatest advantage for a long range plan would be to have an agreed upon level of effort for the program for two or three years in advance, arrived at by some long term process such as discussed above, or as outlined in Appendix F. These plans and budgets should have a fair degree of firmness, but include a concurrent flexibility to meet unanticipated economic and scientific developments.

Glossary of Technical Terms

AGS	The Alternating Gradient Synchrotron, a 30 GeV fixed-target proton accelerator located at Brookhaven National Laboratory, Long Island.
anti-matter	Material made up of anti-particles.
anti-particle	A particle having the same mass as a given fundamental particle, but whose other properties, while having the same magnitude, may be of opposite sign. On collision a particle and its anti-particle can annihilate one another and convert their total energy into other forms.
bubble chamber	A device for detecting charged particles. When a charged particle passes through a bubble chamber a series of bubbles can be produced along its path to yield information on the energy and momentum.
center-of-mass energy	The energy available in a collision to make new particles--the effective energy of a collision, subject to conservation laws.
CERN	The European Organization for Nuclear Research, located in Geneva, Switzerland. It has a 30 GeV proton synchrotron, unique 30 GeV x 30 GeV proton-proton colliding beams - Intersecting Storage Rings, and a new 400 GeV proton synchrotron nearing completion.
colliding beams	A means of attaining very high energy collisions by accelerating two beams of particles and colliding them head on.
cross section	A measure of probability that an interaction will take place expressed in units of area.
cryogenics	The science that deals with very low temperatures and their effect on the properties of matter.
DESY	A 7.5 GeV electron synchrotron laboratory located at Hamburg, West Germany.
deuteron	A hydrogen atomic nucleus consisting of one proton and one neutron. This isotope of the hydrogen atom with oxygen forms "heavy" water.
DORIS	A 4.5 GeV x 4.5 GeV electron-positron colliding beam facility at DESY.

electromagnetic interaction	The electric and magnetic forces that all charged particles experience; transmitted by the photon.
electron (e^-)	A stable elementary particle carrying a negative electric charge of 1.602×10^{-19} coulomb and having a mass of 9.109×10^{-31} Kg($\approx 5 \times 10^{-4}$ GeV). The electron is responsible for normal electrical conduction, and is one of the constituents of atoms.
electron volt (ev)	A unit of energy equal to the energy an electron gains when accelerated across a one volt potential gap.
energy saver/doubler	An R&D project at FERMILAB for a ring of superconducting magnets in the main accelerator tunnel. If successful, this would result in a major reduction in the amount of electric power required for 400 GeV operation and also would permit a doubling of the field strength of the magnets (without increased power need), thereby doubling the energy of the FERMILAB synchrotron.
FERMILAB	The Fermi National Accelerator Laboratory at Batavia, Illinois. The accelerator at FERMILAB accelerates protons to a maximum energy of 400 GeV.
fixed-target accelerator	A machine which accelerates a beam of particles and collides it with a stationary target.
FRASCATI	Italian National Laboratory just outside Rome. It has an electron accelerator and a 1.5 GeV x 1.5 GeV electron-positron colliding beam facility.
GeV	10^9 electron volts (Gigavolt), a unit of energy.
hadron	Any particle which is capable of interacting with the strong force; e.g., protons, neutrons, mesons, etc.
ISABELLE	A proposed proton-proton colliding beam facility at Brookhaven National Laboratory. Each proton beam would have a maximum energy of 200 GeV.
ISR	The Intersecting Storage Rings at CERN, the only proton-proton colliding beam facility in the world. The peak energy of each beam is 30 GeV.
LASS	Large Aperture Solenoid Spectrometer; an experimental facility at SLAC for studying large angle scattering processes.

lepton	A generic name for a class of light particles which have no strong interactions. The known leptons are the electron, muon, and their neutrinos.
magnetic monopole	The magnetic counterpart of the single electric charge; as yet undetected.
meson	A strongly interacting particle (hadron) which is emitted and abosrbed by nucleons and other hadrons. The mesons are thought to transmit the strong force, in the same way that the photon transmits the electromagnetic force.
muon (mu)	An elementary particle with a mass of about 200 times that of an electron, but with all other properties apparently the same as an electron.
neutral currents	The flow of uncharged particles to form a current. A recently discovered phenomenon in which a neutrino (a neutral lepton) scatters off matter and remains a neutral particle. In the more frequent "charged" current processes a neutrino is transformed into a charged lepton (a muon or an electron) when it collides with matter.
neutrino	A massless particle emitted with the speed of light in some decay processes. It interacts with other particles via the weak interaction.
nucleons	A collective name for protons and neutrons.
PEP	Positron-Electron Project, a proposed positron-electron colliding beam facility at SLAC. Each beam would have a maximum energy of 15 GeV.
photon	The carrier of electromagnetic energy. At lower energies these carriers make up infrared waves, light, ultra violet waves, and x-rays. At higher energies photons are known as gamma rays.
proton	The nucleus of a hydrogen atom and a constituent of atomic nuclei in general. It is a stable elementary particle with a positive charge equal and opposite to that of an electron and a mass of 1.673×10^{-27} Kg($\approx$ 1 GeV).
quark	A postulated set of constituents of the elementary particles suggested by the regularities seen in the families of elementary particles.

scaling	A phenomenon whereby the energy dependence of the probability of particle interactions takes on a particularly simple behavior.
SERPUKHOV	The location of the largest Soviet proton accelerator. Its maximum energy is 76 GeV.
SLAC	The Stanford Linear Accelerator Center. The linear accelerator at SLAC accelerates electrons to a maximum energy of 23 GeV.
SLED	An improvement project to increase the maximum electron beam energy at SLAC to 45 GeV.
SPEAR	The 4.2 x 4.2 GeV electron-positron colliding beam device located at SLAC.
SPS	The 400 GeV proton synchrotron presently under construction at CERN.
strong interactions	The enormous forces between particles which hold the nucleus together, probably transmitted by the mesons.
superconductivity	The phenomenon that enables certain metals at very low temperatures to become perfect conductors. It makes possible the construction of accelerator components with very high currents and very low power needs.
TeV	10^{12} electron volts, a unit of energy.
W particle	An as yet unobserved, very heavy particle, which is postulated to be the carrier of the weak interaction.
weak interaction	The force responsible for neutrino interactions with matter and for long lived particle decays, possibly transmitted by the postulated W particle.
ZGS	The Zero Gradient Synchrotron at Argonne National Laboratory. This synchrotron accelerates protons to a maximum energy of 12.5 GeV.

Appendix A

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF MANAGEMENT AND BUDGET
WASHINGTON, D.C. 20503

U.S. ENERGY RESEARCH &
DEVELOPMENT ADMIN
OFF. ADMINISTRATOR

April 9, 1975

1975 APR 10 AM 11 12

MEMORANDUM FOR DR. ROBERT C. SEAMANS

FROM : HUGH F. LOWETH

SUBJECT: High Energy Physics (HEP)

The FY 1976 allowance letter of February 3, 1975 cited the need to reassess priorities of the potential major capital projects for HEP. We are now prepared to initiate the analysis proposed in the allowance letter and to investigate related issues concerning alternative levels of support for HEP operations and the long term facility posture for HEP.

The main issue is: "What should be the long-term strategy toward the construction and operation of HEP facilities?" Three subissues that need to be addressed include:

- To what extent, if at all, has the lack of agreed-on long range planning and budgeting for HEP resulted in inefficient use of available facilities and other resources? How would the present yearly review process be improved by longer range plans based on alternative funding scenarios?
- What should be the long range (i.e., 1985) facility posture of the U.S. in HEP? How will this posture be affected by the high cost of HEP research and by the international nature of HEP research.
- In light of the results of the above issues, what are the priorities of the proposed major construction projects?

We have attached a statement of an internal OMB work plan item which provides some additional detail including our proposed study approach. We ask your support in completing these studies and would like to meet as soon as possible with your staff in preparing final study plans.

Attachment

cc: Dr. Stever, NSF

3/26/75

Work Task Description - Thomas #1

HIGH ENERGY PHYSICS (HEP) PROGRAM

Statement of Issue

What should be the long-term strategy toward the construction and operation of HEP facilities?

Background and Significance of Issue

Proposals have been made by the scientific community for the construction of several new HEP research facilities. These include the energy doubler at the Fermilab, the positron-electron storage rings (PEP) at the Stanford linear acceleratory laboratory (SLAC), a proton-proton storage rings (ISABELLE) at the Brookhaven National Laboratory (BNL), proton storage rings at Fermilab as well as other advanced facilities.

R&D on the energy doubler was initiated in FY 75 using remaining funds from the construction authorization of Fermilab. The FY 76 request by ERDA for construction funds for PEP was denied by OMB. ERDA has also requested OMB agreement to fund construction definition studies on ISABELLE.

Statement of Objectives

Before the FY 77 budget review, it is important to:

- illuminate the impact of possible future funding levels on HEP,
- develop a strategy with respect to the construction of new facilities for HEP research in light of potential availability of funding and priorities within HEP.

Approach/Milestones

First step will be to further define study objective and formulate study criteria and approach (by March 28).

- Request ERDA to undertake study and discuss study plan with ERDA to improve definition (by April 4).
- Request draft response from ERDA by July 30, 1975.

End Product

- Impact analysis of funding alternatives on HEP.
- Priorities for construction of HEP facilities.
- OMB analysis of ERDA study (by 8/31/75) and issue paper laying out future funding strategies by 9/15/75.

Staff Time

10 days.

Related Travel

Intent is to visit existing major facilities and be briefed on future plans.

	Days
Travel would involve two persons visiting two or three HEP facilities (e.g., Fermilab, SLAC or BNL).	3-4

[Note: Appendixes B and C of this document are not included here. Appendix B is the July 30, 1974 "Report of the Subpanel on New Facilities of the High Energy Physics Advisory Panel to the Atomic Energy Commission." Appendix C is the July 1975 "Report of the 1975 Subpanel on New Facilities of the High Energy Physics Advisory Panel to the Energy Research and Development Administration." These are printed as separate documents later.]

Appendix D

International Aspects of High Energy Physics

No discussion of the future of high energy physics in the United States would be complete without some discussion of the large accelerator facilities in other countries. Research in high energy physics has been international in scope from its origins, in the study of cosmic rays and nuclear forces. Exchanges of both theoretical and experimental ideas have been facilitated by open publication and frequent international conferences. In addition, joint experimental work has been common since the pioneering balloon launches of the early cosmic ray physicists.

These traditions have been preserved. New discoveries are quickly transmitted world-wide for both confirmation and extension. Close interaction of physicists on a global scale has enhanced the effectiveness of the work in every country, and has made a major contribution to the development of the field. At the present time, since the U.S. program is almost without redundancy, independent confirmation of many new experimental discoveries comes most readily from work in other countries.

A relatively recent phenomenon has been the collaborative use of foreign facilities--notable examples are the U.S. and European collaborations with the USSR at Serpukhov, the U.S.-Western European collaborations at the ISR at CERN, and the US-USSR collaborations at FERMILAB. This trend will undoubtedly increase in the future, as the high costs of research in this field further restrict redundancy, even on a global scale.

Accelerator facilities will increasingly have an international mixture of users, and scientists from all nations will have to depend more often on facilities of other nations.

A listing of all the high energy physics facilities in the world is shown in Table D-1 according to type of beams (electron-positron colliding beams, proton-proton colliding beams, proton on fixed target, and

Table D-1

HIGH ENERGY PHYSICS PROGRAM
HIGH ENERGY ACCELERATORS

	UNITED STATES	WESTERN EUROPE AND JAPAN	USSR
PROTON SYNCHROTRONS	FERMILAB, 200/400 GEV BNL, AGS 33 GEV ANL, ZGS 12 GEV	CERN II SPS 400 GEV* CERN PS 28 GEV TSUKUBA, JAPAN 12 GEV* RHEL, NIMROD 7 GEV SACLAY, SATURNE 3 GEV LUND, SWEDEN 1 GEV	SERPUKHOV, IHEP 76 GEV DUBNA, JINR 10 GEV MOSCOW, ITEP 7.2 GEV
ELECTRON ACCELERATORS	SLAC 22 GEV CORNELL 12.5 GEV	HAMBURG, DESY 7.5 GEV DARESBURY, NINA 5 GEV BONN 2.3 GEV ORSAY 2 GEV FRASCATI 1.5 GEV TOKYO, JAPAN 1.3 GEV	EREVAN 6.1 GEV KHARKOV 1.8 GEV TOMSK 1.5 GEV LEBEDEV 1.5 GEV
		COLLIDING BEAM SYSTEMS	
PROTON STORAGE RINGS		CERN ISR 30 x 30 GEV PROTON-PROTON	NOVOSIBIRSK VAPP 4 25 x 25 GEV* PROTON/ANTIPROTON*
ELECTRON-POSITRON STORAGE RINGS	SLAC SPEAR 4.1 x 4.1 GEV (LBL/SLAC PEP 18 x 18 GEV)**	HAMBURG, DORIS 5 x 5 GEV ORSAY, DCI 1.8 x 1.8 GEV FRASCATI, ADONE 1.5 x 1.5 GEV ORSAY ACO 0.5 x 0.5 GEV (HAMBURG, PETRA 15 x 15 GEV) **	NOVOSIBIRSK VEPP-4M 7 x 7 GEV* NOVOSIBIRSK VEPP-3M 3.5 x 3.5 GEV NOVOSIBIRSK VEPP-2M 0.7 x 0.7 GEV

* UNDER CONSTRUCTION
** PARTIALLY AUTHORIZED

electron on fixed target). In addition to the proposed facilities shown as partially authorized, other high energy proposals on the world scene include a proton storage ring - accelerator complex in Japan, higher energy proton storage rings at CERN, a higher energy proton accelerator at Serpukhov in the USSR, and the ISABELLE colliding beam facility proposed at BNL.

The evolution of the field of high energy physics has been from a number of small university or regional facilities to a few large central laboratories and national facilities. This has also evolved to international facilities such as CERN in Western Europe and Dubna in Eastern Europe, and to cooperative experiments with national facilities, and may eventually lead to global collaborations in laboratory construction and operation as well as in research. It is conceived that the next step in fixed target machines beyond the multi-TeV machine projected here for FERMILAB would require a cooperative international effort. An international study group currently is being set up to look at the potential problems that would be involved in such a major technological and scientific collaborative effort. Such a major project is probably beyond the ten years of this projection.

Appendix E

Studies Relating to High Energy Physics

1. "Report of the 1975 Subpanel on New Facilities of the High Energy Physics Advisory Panel" (F. Low-Chairman) July 1975.

2. "Report of the Subpanel on New Facilities of the High Energy Physics Advisory Panel (V.F. Weisskopf-Chairman) July 30, 1974.

3. "Report prepared by the Subpanel on the Health of High Energy Physics of the High Energy Physics Advisory Panel" (D. Cline-Chairman) October 1974.

4. "Report of the AEC-NSF Study of the Future Role of the Zero Gradient Synchrotron" (R.C. Drew/J.M. Teem-Co-Chairmen) September 9, 1974.

5. "Long-Range Research Review for the Atomic Energy Commission by the General Advisory Committee" March 1974.

6. "Impact of Shutdown of the Zero Gradient Synchrotron and the Bevatron" (High Energy Physics Program, Division of Physical Research) May 8, 1973.

7. "High Energy Physics: Capital Needs and Accelerator Utilization" (High Energy Physics Program, Division of Physical Research) January 1973.

8. "Report prepared by the Subpanel on Future Patterns of High Energy Research of the High Energy Physics Advisory Panel" (J. Sandweiss-Chairman) July 1972.

9. "Physics in Perspective, Vol. I," National Academy of Sciences (D.A. Bromley-Chairman) 1972.

10. "Physics in Perspective, Vol. II, Part A," National Academy of Sciences (R.G. Sachs-Chairman) 1972.

11. "Considerations for a Viable and Productive High Energy Physics Program" (High Energy Physics Program, Division of Physical Research) January 1972.

12. "Advanced Accelerator Concepts and Technology by the High Energy Physics Advisory Panel Subpanel on Advanced Accelerator Technology" (B. Cork-Chairman) September 1971.

13. "Computer Usage in High Energy Physics by the High Energy Physics Advisory Panel" (H. Taft-Subpanel Chairman) August 1970.

14. "Report on High Energy Physics by the High Energy Physics Advisory Panel" (V.F. Weisskopf-Chairman) June 1969.

15. "The Status and Problems of High Energy Physics Today by the High Energy Physics Advisory Panel" (V.F. Weisskopf-Chairman) January 1968.

16. "Physics Survey and Outlook: A Report on the Present State of U.S. Physics and Its Requirements for Future Growth by the Physics Survey Committee," National Academy of Sciences/National Research Council (G. Pake-Chairman) 1966

17. "Physics Survey and Outlook: Reports of the Subfields of Physics Supplementing the Report of the Physics Survey Committee," National Academy of Sciences/National Research Council (R.L. Walker-Chairman, Subpanel on Elementary Particle Physics) 1966

18. "Policy for National Action in the Field of High Energy Physics" (U.S. Atomic Energy Commission) January 24, 1965.

19. "Report of the GAC-PSAC Panel on High Energy Accelerator Physics" (N. Ramsey-Chairman) April 26, 1963.

20. "Report of the GAC-PSAC Panel on High Energy Accelerator Physics" (E.R. Piore-Chairman) December 14, 1960.

21. "U.S. Policy and Actions in High Energy Accelerator Physics" (E.R. Piore-Chairman) November 1958.

22. "Supplement to the Report of the Advisory Panel on High Energy Accelerators to the NSF" (L.J. Haworth-Chairman) August 1958.

23. "Report of Advisory Panel on High Energy Accelerators to the NSF" (L.J. Haworth-Chairman) October 1956.

24. "Report of the National Science Foundation Advisory Panel on Ultrahigh Energy Nuclear Accelerators" (R.F. Bacher-Chairman) May 2, 1954.

Appendix F

Outline of a Possible Long-Range Planning and Budgeting Procedure

The problems resulting from the lack of agreed-on long-range budget plans for high energy physics were discussed in Section VII. The "Bannier Procedure" which is used for budget planning at CERN was cited as an example of a successful system of long-range planning. This Appendix discusses, as an example, a variation on such a system which might be compatible with U.S. budget procedures. It would involve agreed-on plans between the OMB and the agencies supporting high energy physics (ERDA and NSF).

The U.S. procedure could consist of three parts: (1) a long range plan which would project over a period of perhaps ten years, to be used as program guidance; (2) a shorter range plan of three or four years which would be the basis of the agreed-on plans; and (3) the annual budget for the next fiscal year. The three plans would differ in degree of firmness and in the level of commitment that would be made by the OMB and the agencies, as described below:

1. Long-Range Plan. The long-range plan would cover a period of about ten years and would be developed from information similar to that in this report. Its purpose would be to serve as guidance, both for future development of the program and for future funding needs. This would include, of course, both operating and construction guidelines. The substance of the plan would be thoroughly reviewed, and the plan updated approximately every two years, or as required.

2. Short-Range Plan. The short-range plan would cover the first two to four years of the long-range plan. The first two years of the short-range plan would be considered firm. (The "Bannier Procedure" covers a four year period--two of which are firm and two provisional.) The Short-Range Plan would be developed under the general guidelines of the Long-Range Plan, but would involve careful examination and

evaluation of specific requests and justifications. The short-range plans could be developed as part of the regular annual budget process between the funding agencies and the OMB. The resulting agreed-on budget plans would be in terms of constant value dollars.

3. The Annual Budget. The annual budget would be based on the budget level which was firmed up the previous year in the Short-Range Plan, with actual funding adjusted for cost index changes. Modifications from the agreed-on level from the previous year's Short-Range Plan would be made only on the basis of unforeseen technical and economic developments. This would include consideration of any changes introduced in the prior year's budget by the Congress.

THE U. S. ENERGY RESEARCH & DEVELOPMENT ADMINISTRATION
REPORT OF THE HIGH ENERGY PHYSICS ADVISORY PANEL
1975 SUBPANEL ON NEW FACILITIES

JULY 1975

Yale University *New Haven, Connecticut 06520*

PHYSICS DEPARTMENT
217 Prospect Street

July 25, 1975

Dr. Daniel R. Miller
Acting Director, Division of Physical Research
Energy Research and Development Administration
Washington, D. C. 20545

Dear Dr. Miller:

I am forwarding to you the report of the 1975 HEPAP Subpanel on new facilities. This report is in response to the charge to study the new facility needs of the U.S. high energy physics program given to HEPAP by Dr. John Teem of ERDA and Dr. Marcel Bardon of the NSF at the May 16-17, 1975 meeting at SLAC.

The report was discussed by HEPAP at its July 17 meeting in Washington and the conclusions and recommendations of the Subpanel were fully endorsed. The Panel agrees unanimously with the Subpanel's recommendation of the need for a three pronged approach to the highest energies via electron-positron colliding beams, proton-proton colliding beams, and protons colliding with fixed targets. PEP is a critical feature in this approach and there is complete agreement with the recommendation that the PEP facility be authorized in FY 1976 and, if that is not possible, that it should be given highest priority for FY 1977. PEP represents an outstanding combination of physics potential, technical readiness, and laboratory strengths.

The great importance of the high energy of PEP is primarily related to two fundamental physics goals. First, this energy (15 GeV per beam) would allow for the first time a systematic study of the interference between electromagnetic and weak processes, which is strongly energy dependent and requires the full energy of PEP. Since electromagnetism is so well understood, this effect provides an ideal way of studying the fundamental structure of the weak interactions and may indeed reveal a common origin of the weak and electromagnetic forces. If so, this would be one of the great unifications of apparently diverse phenomena, on a par with Maxwell's unification of electromagnetism and light. Second, the high energy would permit an extensive search for new particles which can be formed in the electron-positron collisions. The past year has seen a dramatic series of such discoveries and it is of great importance to extend the search to the energies which could be reached by the PEP facility.

Given the strong need for the three pronged approach to higher energies, the importance of attaining the highest electron-positron colliding beam energy, and a sense of balance for the program, it was not considered appropriate, under the three constrained funding alternatives, to recommend construction authorization of the Cornell electron-positron colliding beam project. The Cornell proposal was judged to be technically sound and the physics interesting. However, it cannot reach the energies which, for the reasons described above, are critically important.

There is a shared concern about the level of utilization of our existing facilities and about the physics research opportunities which would be realized if more optimum levels were achieved. Nevertheless, in considering the balance in the program between the need for higher utilization and the need for new facilities it was clear to HEPAP that the intrinsically exploratory character of high energy physics research makes it vitally important to extend the range of our accelerator capabilities.

Finally, I would like to take this opportunity on behalf of all the members of HEPAP to express our gratitude and appreciation for the work of the Subpanel.

Sincerely,

Jack Sandweiss

Jack Sandweiss
Acting Chairman,
High Energy Physics Advisory Panel

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

June 30, 1975

Dr. Sidney Drell
Chairman,
High Energy Physics Advisory Panel
c/o Stanford Linear Accelerator Center
P. O. Box 4349
Stanford, California 94305

Dear Sid:

I enclose the Report of the 1975 Subpanel on New Facilities which met in Woods Hole between June 15 and June 21.

Since this may not be the last such Subpanel, I would like to comment on the mode of operation and scheduling of such meetings.

First, it may be that for the particular problem of examining several proposals which appear in May and which have to be inserted into the budget cycle in July, the month of June is uniquely selected. In addition to the above scheduling reasons, this is because those of us who have teaching responsibilities cannot easily leave them until the end of May, and also usually have long-standing summer commitments for July and August. Of course, with the new fiscal year these two requirements may no longer coincide.

Second, I view the single week of hard work as a very good format. However, a schedule that requires an essentially completed report (except for telephoned changes) by the end of that week is too tight. There should be time for the participants to leave, think calmly about their tentative conclusions, discuss them with their colleagues and with interested parties, and then reassemble for two or three days to work out a final report. This year, for example, this would have put the one week meeting at the beginning of June, with the final two or three days toward the end of the month, and could have been planned for had the Subpanel been organized a month or two earlier. Since I believe the agencies generally have some advance notice of proposals, this should be possible in the future.

With best regards,

Yours sincerely,
Francis
Francis E. Low

FEL:dle

CHARGE TO 1975 SUBPANEL ON NEW FACILITIES

Undertake an updating for the U.S. national High Energy Physics Program of the July 1974 report of the Subpanel on New Facilities[1], assuming the same budgetary constraints (called Level A)[2] and in the light of:

I. The new physics discoveries;

II. The new construction proposals (Cornell Colliding Beams, FERMILAB Energy Doubler/Saver);

III. The present status of all current proposals for new facilities.

Include in the report an assessment of each new facility proposal and its technical status.

Make specific recommendations regarding new facilities for the FY 1977 budget within a long range context for the national High Energy Physics Program.

How would your recommendations be modified under the following average annual funding levels:

Level B - About midway between the constraints given to the Weisskopf Subpanel (Level A) and those of Level C defined below.

Level C - FY 1976 total funding + $20 million (constant value dollars).

Level D - A level that you would recommend as closer to optimum for the promise and needs of the field.

The report should be completed by the end of June.

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[1] In order to avoid confusion in what follows, we shall refer to the 1974 Subpanel as "the Weisskopf Subpanel," and to the 1975 Subpanel as "the Subpanel."

[2] The budgetary constraints given to the Weisskopf Subpanel for the U.S. program in high energy physics: 1) operations and equipment at an annual level of roughly $200 million in FY 1974 dollars, on the average, which is about the average annual level for such spending in the U.S. over the previous 10 year period. (The $200 million is estimated at about $245 million in FY 1976 dollars.) 2) new facility construction--an average of about $40 million per year in FY 1974 dollars over the next 10 year period, which is about 2/3 of the average per year over the previous 15 year period. ($40 million is estimated at about $50 million in FY 1976 dollars.)

Introduction

The 1975 HEPAP Subpanel on New Facilities was formed in May 1975. It held a first meeting at the Fermi National Accelerator Laboratory on May 29 and then met for a week at Woods Hole, Massachusetts, between June 15 and June 21, 1975. The meetings had the following participants:

Panel Members:

F.E. Low, Chairman (MIT)
B.C. Barish (Cal. Inst. Tech.)
K. Berkelman (Cornell)
H. Frisch (Chicago)
D. Keefe (LBL)
B. Lee (FERMILAB)
J.E. Leiss (NBS)
V. Fitch (Princeton)
A.K. Mann (Penn.)
R. Martin (ANL)
A. Pais (Rockefeller)
J.R. Purcell (ANL)
W. Schnell (CERN)
K. Strauch (Harvard)
M. Strovink (LBL)
G.A. Voss (DESY)
R. Woods (ERDA), Exec. Secy.

Special Consultants:

E. Courant (BNL)
D. Edwards (FERMILAB)
W. Gilbert (LBL)
R. Kropschot (NBS-Boulder)
L.J. Laslett (LBL)

Proposal Experts:

A. Silverman (Cornell)
M. Tigner (Cornell)
K. Wilson (Cornell)
M. Barton (BNL)
N. Samios (BNL)
W. Sampson (BNL)
P. Oddone (LBL)
J. Rees (SLAC)
B. Richter (SLAC)
W. Fowler (FERMILAB)
J. Peoples (FERMILAB)
L. Teng (FERMILAB)

Observers and Visiting Participants:

R.L. Fricken (ERDA)
W.B. Herrmannsfeldt (ERDA)
H. Kinney (ERDA)
D. Miller (ERDA)
W.A. Wallenmeyer (ERDA)
A. Abashian (NSF)
D. Berley (NSF)
B. McDaniel (Cornell)
H. Hahn (BNL)
A.F. McInturff (BNL)
R. Palmer (BNL)
R. Rau (BNL)
G. Vineyard (BNL)
W. Panofsky (SLAC)
T. Collins (FERMILAB)
E. Goldwasser (FERMILAB)
N. Ramsey (URA)
R.R. Wilson (FERMILAB)

The meeting was structured as follows: on June 15 the Subpanel and consultants met for preliminary discussions and planning. The next day, the Subpanel heard presentations on the four proposals which it was specifically asked to consider for FY 1977 funding. These were:

- PEP: An electron-positron colliding beam facility designed to operate from 5 to 18 GeV in each beam, with a luminosity of $10^{32} cm^{-2} sec^{-1}$ at 15 GeV, proposed by Lawrence Berkeley Laboratory (LBL) and Stanford Linear Accelerator Center (SLAC).

- ISABELLE: A proton-proton colliding beam facility designed to operate from 30 to 200 GeV in each beam, with a luminosity up to $10^{33} cm^{-2} sec^{-1}$, proposed by Brookhaven National Laboratory (BNL).

- Cornell: Proposal for an electron-positron colliding beam facility designed to operate from 4 to 8 GeV in each beam, with a luminosity of $10^{32} cm^{-2} sec^{-1}$ at 8 GeV.

- Energy Doubler/Saver: A superconducting synchrotron with alternative uses as a proton energy doubler or an electrical energy saver, designed to produce proton beams with energies up to 1000 GeV, at a rate of up to 10^{12} protons per second, proposed by the Fermi National Accelerator Laboratory (FERMILAB).

The Subpanel also heard presentations of the long-range plans and possible future projects of the four laboratories concerned, so that its members could establish the long-range context that was a necessary background to their deliberations. Two days were then devoted to a detailed study of physics and accelerator questions, and the last three days to arriving at recommendations and comments. The consultants participated in the meeting on the first five days and the proposal experts on the second, third, and fourth days.

As stated in the charge (see p.1), this report is to be viewed as an updating of, and addition to, the report of the Weisskopf Subpanel which made the following recommendations:

" I. Authorization in FY 1976 of the LBL/SLAC proposal for the construction at SLAC of a 15 GeV electron-positron colliding beam facility (PEP) with a design luminosity of $10^{32} cm^{-2} sec^{-1}$.

" II. Provision of funds in FY 1976 of $3-4 million for BNL to complete fabrication of prototypes of superconducting magnets, with the aim of making a request for early construction at BNL of a proton-proton colliding beam facility (ISABELLE) with a design luminosity of $10^{33}\mathrm{cm}^{-2}\mathrm{sec}^{-1}$ and a beam energy of at least 200 GeV.

"III. Provision of funds to support an accelerator development program at FERMILAB directed toward the long-term goal of fixed target and/or colliding beam systems in the region of 1000 GeV and above."

The Subpanel was in complete agreement with the long-range goals implied by the above recommendations--the three-pronged approach to the highest energies via electron-positron colliding beams, proton-proton colliding beams, and multi-TeV protons colliding with fixed targets. The Subpanel believes that the steps it has reccommended are appropriate ones for FY 1977 in the light of those goals. It is very aware of the fact that both technology and science can change rapidly and that frequent reassessment may be necessary.

The Subpanel interpreted the alternative funding levels of the charge as follows (in FY 1976 dollars):

Level A (the Weisskopf Subpanel Level):

$245 million operations and equipment
50 million construction

Level B:

$250 million total

Level C:

$190 million operations and equipment
20 million construction

Level D:

The Subpanel did not attempt to set a dollar figure on this Level.[1]

In addition to the specific proposals for FY 1977, the Subpanel discussed various additional capabilities, such as electron-proton and proton-antiproton colliding beams, higher energy colliding beam systems, and fixed target accelerators.

In writing this report an attempt has been made to keep each section self-sufficient by freely repeating material where needed.

The recommendations given in Section IV refer exclusively to the funding Level A. The alternative funding levels are considered in Section V.

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[1] This was largely because in the single week that the Subpanel had at its disposal, it was unable to give as much time to this Level as would have been needed to do so. F. Low

Section I

New Developments in Particle Physics

It is only one year since the Weisskopf Subpanel held its meeting, yet in this short time several fundamental discoveries have been made in high energy physics which have greatly excited the physics community. These discoveries were made in three large U.S. high energy physics centers--BNL, SLAC, and FERMILAB--using both colliding beams and fixed target machines. Here we shall briefly outline what has happened in this period.

First, new forms of matter have been discovered, which have been named Ψ or J particles. They were revealed both in proton beam experiments at BNL and in electron-positron colliding beam experiments at SLAC, and more recently have also been observed in photoproduction experiments at FERMILAB, SLAC, and Cornell. Their masses lie in a new range, between three and five times the mass of the proton. These new particles are particularly intriguing in that their decays proceed very much more slowly than one would expect for such heavy objects. It is this circumstance which had led to the broadly accepted view that these particles signal the presence of a hitherto unobserved fundamental attribute of matter, analogous to "Strangeness" which was discovered in the early 1950's, and of comparable import. The new particles may provide the key to certain puzzles in the weak interactions, and show promise of leading to a deeper understanding of the relations among the different basic forces in nature.

One can describe this new attribute of matter in another language. It is a fruitful working hypothesis to assume that the known strongly interacting particles, the hadrons, consist of elementary subunits, the "quarks." The existence and properties of the newly discovered particles strongly suggest that there are more such subunits than was commonly supposed.

A second basic discovery points in the same direction. During the past year, SLAC has extended to higher energies the investigation of the annihilation of electron-positron pairs into hadrons and has found the rate to be unexpectedly high. This rate is essentially a measure of the number of different kinds of quarks in nature and indicates once again that there are more fundamental subunits than was originally thought.

The past year has seen additional new discoveries. Experiments with neutrino beams both at FERMILAB and at BNL strongly indicate still other new forms of matter, possibly related to the Ψ or J particles.

While it is too early to say precisely what all these phenomena signify, one thing is clear: In all our major laboratories, whether one works with proton, electron, photon, or neutrino beams, there is indication of a potential breakthrough wherever one looks.

Last year the Weisskopf Subpanel concluded its introductory comments as follows: "It is difficult to express in guarded terms the enthusiasm generated in the high energy physics community by the prospects of such major breakthroughs." Now, one year later, we are actually witnessing the beginnings of some of those breakthroughs, which give additional impetus and urgency to the construction of new facilities to open up new energy regions for exploration.

Section II

Physics at the Proposed New Facilities

The discoveries of this past year provide an example of the fact that progress in our understanding of the fundamental properties of matter requires the use of both hadron and lepton probes. The information gained with each is complementary and permits a unified approach to the problem of finding the elementary constituents of matter and of understanding their interactions. We endorse the long-range goals expressed in the 1974 report that "the energy frontier should be pushed forward with positron-electron colliding beams, with proton-proton colliding beams, and with a fixed target proton accelerator of at least 1000 GeV." It is with these goals in mind that we have examined the proposals before us and formulated our recommendations for the immediate future.

Electron-Positron Colliding Beams. The positron and electron beams colliding in a storage ring provide the source of an initial state of particular simplicity, uncomplicated by strong interactions. Such an initial state may materialize into states containing one or more new particles. In addition, the electromagnetic force describing the electron-positron interaction is unique among the fundamental forces in that we have an extraordinarily accurate theoretical apparatus for its understanding. In other words, many of the cross sections of varying degrees of complexity can be calculated with confidence. For this reason, the electron-positron colliding beam facility is an ideal tool to look for new particles, as the recent discoveries at SPEAR attest. The distribution and yields of hadrons produced in electron-positron collisions give essential information on the character of the internal constituents of hadrons. Studies of electron-positron scattering, and the production of muon pairs in electron-positron collisions, incisively test the validity of quantum electrodynamics as a theory of high energy electromagnetic interactions. Furthermore, at center-of-mass energies of about 15 GeV and above, certain effects of weak interactions are expected to be clearly observable in these processes. Using electron-positron annihilation, in this energy domain, it will be possible to probe the workings of the weak force in a completely new setting other than in decays of particles and neutrino interactions.

PEP is designed to operate from 5 to 18 GeV in each beam, with a luminosity of $10^{32}\text{cm}^{-2}\text{sec}^{-1}$ at 15 GeV. With later additions, electron-proton collisions could be achieved. The Cornell facility is designed to operate from 4 to 8 GeV in each beam with a luminosity of $10^{32}\text{cm}^{-2}\text{sec}^{-1}$ at 8 GeV.

Proton-Proton Colliding Beams. Proton-proton colliding beams provide the highest reaction energies, which would be prohibitively expensive if obtained by any other means. In return for this giant leap in energy, however, a price is paid in that the variety of collisions that can be studied by this means is restricted to interactions of the accelerated protons. With later modifications, electron-proton, deuteron-proton, and antiproton-proton collisions could also be achieved.

The basic characteristics of the strong interactions of protons can be studied at the highest attainable energies with this tool. It is important to know whether the rising total cross section observed at Serpukhov, the CERN Intersecting Storage Rings (ISR), and FERMILAB continues to grow at still higher energies. One can, in addition, probe the inner structure of hadrons by using head-on collisions at energies higher by an order of magnitude than available at present.

At center-of-mass energies above a few hundred GeV and at large angles, the strength of the weak interactions may become comparable to that of the strong interactions. Therefore, proton-proton colliding beams in the hundred GeV range promise to be an effective way of observing new manifestations of weak interaction effects.

Since massive particles, such as the hypothetical W particle, can only be produced at energies above a minimum threshold, a search for such particles benefits from a device capable of producing the highest attainable energy. Finally, if history is a guide, we may expect completely unforeseen phenomena to appear in a new domain.

The proton-proton colliding beam facility ISABELLE is designed to operate at energies of 30 to 200 GeV in each beam, with a luminosity up to $10^{33}cm^{-2}sec^{-1}$.

Fixed Target Accelerator. A fixed target accelerator, while not attaining the high effective center-of-mass energy of a colliding beam machine, has the virtue of a large interaction rate, since the fixed target consists of dense matter rather than of a diffuse beam of particles. Thereby, intense secondary beams of various particle types can be produced, which allow us to study strong interactions of π-mesons, K-mesons, neutrons, etc. with matter in addition to the proton-proton interactions. In this way, the inner structure of hadrons other than the proton can be explored. It is also possible to produce neutrino, electron, muon, and photon beams. Such beams are unique probes for studying weak and electromagnetic interactions of hadrons and the properties of their internal constituents.

The Energy Doubler/Saver would enable the FERMILAB accelerator to produce beams of up to one thousand GeV at a rate of up to 10^{12} protons per second. This new facility not only would increase the range of energies that can be explored at FERMILAB but also would serve as a step in the development of facilities at that laboratory which could produce still higher energies.

Section III

Technical Considerations

In the proposed PEP(1) project, colliding beams of 5 to 18 GeV electrons and positrons, injected from the SLAC accelerator, are made to interact in six interaction regions (five of which are available for experiments) with an anticipated maximum luminosity of $10^{32}\mathrm{cm}^{-2}\mathrm{sec}^{-1}$ at 15 GeV. The SLAC accelerator has the capability of delivering the high energy, high intensity electron and positron beams for injection at full energy into the PEP device. The design allows possible addition, in the future, of a proton ring which would allow electron-proton collisions in an extremely interesting domain of both energy and momentum transfer.

The PEP proposal is the result of an extensive study at SLAC and LBL that led to a detailed design report which contains a thorough analysis of the expected performance and a description of construction plans. The proposed facility is a logical extension of the existing highly successful program at SLAC. The extensive experience gained in the operation of SPEAR has contributed significantly to the degree of assurance one can have in the PEP performance estimates. The PEP design relies largely on proven technology, and as presented appears to the Subpanel to be a sound basis for construction that can start now.

In the past year some improvements have been incorporated in the design. In particular, the magnet lattice has been modified by the addition of extra focussing elements which allow the option of extension in energy from 15 GeV to some 23 GeV per beam at high luminosity by the future addition of rf power. The question of rf power losses in excitation of higher order modes has been further examined extensively both experimentally (at SPEAR) and theoretically; as a result the requirements on the vacuum chamber impedance have been identified. Also, operational experience with SPEAR has led to a slight change in the magnet aperture so as to accommodate 100% coupling of horizontal and vertical betatron oscillations. Building of prototype dipole and quadrupole magnets has been completed.

In considering the possibility of operating PEP at half energy if the physics made it desirable, the Subpanel concluded that simple rearrangements of some quadrupoles could lead to luminosities close to the peak value.

> The Subpanel feels that the design of PEP is technically sound and mature, that it is ready for an immediate start in construction, and that it can be built on the proposed cost and time schedules with a very high degree of confidence.

The Cornell proposal(2) for an electron-positron colliding beam facility (4-8 GeV per beam) is a new one. In this proposed facility, colliding beams of electrons and positrons, injected from the Cornell synchrotron, are made to interact in two interaction regions which are available for experiments with an anticipated maximum luminosity of $10^{32} cm^{-2} sec^{-1}$ at 8 GeV. The design allows future addition of more rf power to raise the peak energy to 10 GeV per beam.

The design appears sound although it is still being refined. An ingenious injection scheme is proposed for stacking beam in the storage ring to allow use of the existing Cornell synchrotron as an injector. This places more sophisticated requirements on the injection system than, for example, in PEP, but it will probably work satisfactorily. The technical basis for design is essentially the same as for PEP and the technical problems similar. Also the estimates of cost for the two projects seem to have been made in a comparable manner.

Polarization time constants for both the PEP and Cornell beams operating at 15 GeV and 8 GeV, respectively, seem adequately short so that experiments with transversely polarized beams are possible.

> In the judgment of the Subpanel, the technical status of the Cornell proposal is such that construction could begin in FY 1977 with a high degree of confidence of success.

ISABELLE(3) is a proposed proton-proton colliding beam facility. The only existing device of this kind is the ISR at CERN. ISABELLE constitutes a natural next step in this type of facility. Protons from the existing 30 GeV accelerator (AGS) at BNL would be injected and stacked in ISABELLE and then accelerated to a maximum energy of 200 GeV. Possible future additions might include the capability to collide protons with other particles, in particular electrons or antiprotons. As has been stated in the Weisskopf report, the ISABELLE proposal is the result of several years of detailed study and of following the development of, and the experience gained with, the ISR. This study, which has actively continued during the last year, covers all known pertinent aspects of accelerator physics, and confirms the conclusions of the Weisskopf

Subpanel, viz., that there is a sound basis for proceeding into the construction phase. The estimated maximum luminosity of $10^{33}cm^{-2}sec^{-1}$ is above the maximum so far achieved in the ISR by a factor of 50. Convincing evidence is given that this luminosity can be achieved after a period of running-in.

In the past year some improvements have been incorporated in the design. The magnet lattice has been modified in order to overcome difficulties in accommodating protons at the required momentum spread. The new lattice is simpler than the old one, and it appears that the change should lead to greater ease of operation, improved flexibility of experimentation, and increased performance potential. The new lattice also led to an increase from 4 to 7 in the number of possible colliding beam regions. Acceleration of an intense beam in the colliding beam device itself has been carefully studied by BNL and a special rf system is planned. The Subpanel agrees with the conclusion presented in the study report, that acceleration as required can be achieved. The capability for acceleration permits the entire energy range from 30 to 200 GeV to be covered at full circulating current.

A very important aspect of the design is the proposed use of superconducting magnets to enhance the potential performance and to reduce power consumption. In this respect, the Weisskopf Subpanel expressed the belief that the field of superconductivity is now sufficiently advanced so that the technology can be applied in a project like ISABELLE. The Weisskopf Subpanel also felt, however, that the time allowed to get from the model stage to a state of being ready to order such magnet systems on an industrial scale had been underestimated. In the intervening year BNL has built a full-size (both in length and bore diameter) model of their dipole magnet; and a first set of preliminary tests has been successfully carried out. We consider this result encouraging.

> The Subpanel believes that adequate funds, about $3 million, should be made available for ISABELLE R&D in FY 1976. Under the assumption that this is done, the Subpanel feels that ISABELLE can be built on the time schedule and within the cost proposed, although the time schedule for the start of the project is tight. The Subpanel sees no substantive difficulties in the accelerator-physics aspects of the ISABELLE design as proposed.
>
> The Subpanel feels that as the final design progresses, BNL will be able to make improvements in the design, particularly in the cryogenics area.

The Energy Doubler/Saver proposal(4) is a result of the recognition by FERMILAB from the inception of that laboratory that it might be possible to provide for an increase by a factor of two or more in energy by placing a second, superconducting accelerator ring in the tunnel, and using the present accelerator as an injector. The attractiveness of this proposal has been further enhanced by the recognition that substantial power savings might also result from such an accelerator, leading to potential reduction in overall energy consumption of the laboratory in addition to the increased potential for physics research. Space for this possible addition to the facility was made in the original laboratory design and construction.

The additional ring of superconducting magnets could give twice the magnetic field of the existing magnets and hence double the energy, or could produce about the same intensity and energy as the present accelerator with a saving of as much as $5 million per year or more in electric power costs were the existing machine to run at its maximum capability. As now contemplated, in addition to the use of the Energy Doubler/Saver as an accelerator, it could be operated as a beam-pulse-stretcher for improved duty cycle at electrical power consumption substantially below that now necessary for operation of the existing accelerator.

The proposal assumes extensive research and development during FY 1976, and seeks construction authorization in FY 1977. During FY 1977 all accelerator and systems components would be constructed and installed. Systems testing would be conducted during FY 1978 with the experimental physics program starting at the beginning of FY 1979. It is proposed that groups of magnets can be introduced into the accelerator tunnel during normal operations of the present accelerator, with transport in the tunnel, installation, and alignment occurring during normal maintenance periods.

Assuming the FY 1976 R&D program can be carried out in a satisfactory manner, the cost of the project is estimated by FERMILAB to be $35 million with $25 million to be costed in FY 1977 and $10 million to be costed in FY 1978.

Studies of the Energy Doubler/Saver concept have been carried out during the last several years, and have included initial investigation of overall accelerator systems and installation questions, beam dynamics, cryogenics and refrigeration systems, and a magnet development program. Much of the effort during the last year has been on research directed toward suitable superconducting magnets. These studies are still in progress and are planned for continuation in FY 1976.

The Subpanel notes that the character of both the material and the presentations describing the Energy Doubler/Saver proposal is of a different order of detail from the other proposals evaluated--and therefore its evaluation must reflect this difference. At this stage the proposal resembles a large accelerator R&D project. This is true, for example, in that the final performance expected is not clearly defined. This in itself is not a strong criticism since if only some of the hoped-for goals are realized, the project would have payoffs in the particle energy achieved, the savings in electrical power, and the new technology developed. The Subpanel feels that the technology to build an Energy Doubler/Saver is advancing rapidly, and that when this technology reaches an appropriate state, an Energy Doubler/Saver can be built which reaches the ultimate goals defined for the project.

The R&D efforts of FERMILAB directed toward the Energy Doubler/Saver are moving very rapidly and are making substantial progress. At this time, achievement of adequate magnets has not been demonstrated. The R&D program described for FY 1976 hopes to achieve this; however, we cannot judge the probability of success. If the objective of one thousand GeV were relaxed to a lower peak energy, the probability of achievement of suitable magnets in the FY 1976 R&D program would be increased. It appears probable that FERMILAB will have developed magnets suitable for the Energy Doubler/Saver that can operate reliably at 33 KG (~ 750 GeV) by FY 1977; however, satisfactory training behavior, field quality, ramping capability, etc. remain to be demonstrated. The Subpanel feels that a number of areas such as magnet aperture requirement, beam instabilities, cold-bore operation, vacuum problems, etc. require further study.

The Subpanel feels that the project as described will cost more than proposed.

To have any hope of achieving the schedule as proposed, substantial R&D expenditure in FY 1976 is essential.

> The Subpanel feels that to commit to construction funding in FY 1977 at this time carries substantial risk, and that a later decision on construction funding, after the progress of the FY 1976 R&D program can be assessed, would be more appropriate.

References:

(1)A Proposal for a Positron-Electron Colliding-Beam-Storage-Ring Project (PEP) LBL 2688/SLAC 171 (April 1974).

The PEP Electron-Positron Ring--An Update; PEP Note 108 (March 1975).

Selections from a Draft Addendum to a Proposal for a Positron-Electron Colliding-Beam Storage Project (PEP) (June 1975).

(2)Electron-Positron Colliding Beam Facility at Cornell. A Proposal to the National Science Foundation (May 1975).

(3)A Proposal for Construction of a Proton-Proton Storage Accelerator Facility ISABELLE, BNL 18891 (May 1974).

A Proposal for Construction of a Proton-Proton Storage Accelerator Facility ISABELLE, BNL 20161 (June 1975).

(4)The Energy Doubler/Saver Design Report FN-263, REV (June 1975).

Section IV

Recommendations and Comments[1/]

I. We reaffirm the Weisskopf Subpanel's recommendation for authorization in FY 1976 of the LBL/SLAC proposal to construct the electron-positron colliding beam facility PEP. The facility will operate at beam energies between 5 and 18 GeV with a peak luminosity of $10^{32}cm^{-2}sec^{-1}$ at 15 GeV. If construction funds are not available in FY 1976, we recommend as our highest priority the construction funding of this project in FY 1977.

II. We recommend authorization in FY 1977 of the BNL proposal for the construction of the proton-proton colliding beam facility ISABELLE. This facility is designed to operate at energies between 30 and 200 GeV per beam with a peak luminosity of $10^{33}cm^{-2}sec^{-1}$. The Subpanel also recommends that BNL roughly double its effort in FY 1976 for research and development in superconducting magnets and cryogenic systems and for the further development of prototypes in anticipation of authorization in FY 1977.

III. We reaffirm the Weisskopf Subpanel recommendation that funds be provided to support an accelerator development program at FERMILAB directed toward the long-term goal of fixed target and/or colliding beam systems in the region of 1000 GeV and above, and we recognize the Energy Doubler/Saver as a proper step to this end. We recommend providing funds in FY 1977 at the level of $10 million or more for FERMILAB to support continued research and development directed toward an Energy Doubler/Saver capable of at least 800 GeV in energy and 5×10^{11} protons per second in average intensity.

IV. We do not recommend construction funding in FY 1977 of the proposed 8 GeV electron-positron facility at Cornell. This proposal is technically sound and the facility would unquestionably produce valuable new results in what has proved to be an exceptionally fertile field of physics. Nevertheless, faced with the need to extend high energy physics on three frontiers--electron colliding beam physics, proton colliding beam physics, and stationary target physics--the Subpanel reluctantly felt it could not recommend more than one facility on this particular frontier.

[1/] We remind the reader that the recommendations given in this section refer exclusively to the funding Level A. The alternative funding levels are considered in Section V.

Comments on Recommendation I

In the past year, SLAC/LBL experimenters using electron-positron colliding beams at SPEAR have discovered a family of particles which appear to represent new forms of matter. Many characteristics of these particles have been determined and new insights into the structure and multiplicity of hadron constituents are beginning to be gained. These discoveries reinforce the Subpanel's enthusiasm for the experimental program proposed for the PEP project, for which SPEAR is the prototype. The four times higher energy and broadened experimental scope of PEP will preserve U.S. leadership in this field. The studies of hadron production will be extended over a broad region of higher energies, and new studies of the weak and electromagnetic interactions will become possible.

The PEP project clearly is ready for construction. The proposal estimates the cost of construction as $58 million (FY 1975 dollars). The project is to be built at SLAC as a collaborative effort of LBL and SLAC. If the present level of fixed target research at SLAC is maintained, PEP operations are expected to add $4 million per year to the SLAC operating budget. The new facility will make use of the existing SLAC linear accelerator as a unique injector and of the scientific and engineering talent which exists at both laboratories. The PEP design preserves the option of adding a proton ring for future study of collisions between protons and positrons or electrons.

The Subpanel notes that steps have been taken to involve potential users in the early planning and operation of PEP as a national facility. It feels that the project presents an outstanding combination of physics potential, technical readiness, and laboratory strength.

Comments on Recommendation II

At the present time the ISR with 30 GeV per beam provides the highest center-of-mass energy in the world. The sevenfold increase in energy in the ISABELLE facility is a highly significant step. Such a step opens large energy regions for the production of new particles and the discovery of new phenomena, probing nature at distances much smaller than those previously accessible. This high center-of-mass energy is even more dramatic when expressed as the equivalent fixed target accelerator energy of 86,000 GeV. The design of ISABELLE is such that a full coverage of the energy range from the peak ISR energy to the maximum ISABELLE energy will be possible. New knowledge can be tied, thereby, with what is already known. We note also that the peak design luminosity of ISABELLE is 50 times greater than that so far obtained at the ISR.

As noted by the Weisskopf Subpanel, ISABELLE is the result of several years of detailed study by BNL. It uses the pioneering research and development work on superconducting magnets and transmission lines at that laboratory. It draws heavily on the experience derived from the successful operation of the ISR at CERN. The Weisskopf Subpanel recommended a substantial magnet development effort with the "aim of making a request for early construction" of ISABELLE at BNL. We note the encouraging progress that has been made in this area since this recommendation. The major step in the use of superconducting systems represented by this project will have an important impact in many fields of science and technology, especially because of the power savings that are possible in such systems.

The Subpanel notes two important additional ISABELLE features. First, the storage at 30 GeV followed by acceleration to 200 GeV lends itself to proton-antiproton colliding beams. While the calculated luminosity is relatively low when ISABELLE is used in this mode ($10^{29}cm^{-2}sec^{-1}$), it is still adequate for experiments of great interest. Second, the electron-proton colliding beam capability can be added at a later time. The cost of ISABELLE is estimated to be $160 million (FY 1975 dollars). If the present AGS experimental program were maintained in addition to ISABELLE operation, it is estimated the operating budget would be increased by $17 million per year.

Comments on Recommendation III

The 400 GeV accelerator at FERMILAB is at present the highest energy fixed target accelerator in the world. It supports a large and diverse program utilizing both intense primary beams of protons and a large variety of high energy secondary beams, including beams of π-mesons and K-mesons, neutrons, photons, muons, and neutrinos. This diversity of available particles as probes and the high interaction rate inherent in a fixed target accelerator have already proved enormously productive, and promise even greater potential in the years to come.

The minimum goal of the proposed Energy Doubler/Saver would allow an increase of the available energy from the present 400 GeV to at least 800 GeV, with an average intensity of at least 5×10^{11} protons per second. Possible advances in technology and design could allow higher intensities and energies. This substantial increase in the energy of the machine would make possible the extension of investigations involving beams of secondary particles such as pions or neutrinos into a hitherto inaccessible area. With some modification, the large array of experimental facilities available at FERMILAB would be in a position to take rapid advantage of the increased capabilities of the Energy Doubler/Saver.

The uniqueness of the 400 GeV accelerator at FERMILAB will be affected by the expected operation in Europe of the CERN 400 GeV SPS project. The ability to do physics at energies of 800 GeV would maintain FERMILAB's position at the forefront of the field.

The proposed Energy Doubler/Saver could also be operated in a wide range of modes with the goal of greatly reducing power consumption at FERMILAB while still providing very high energy beams. Possible savings in operating funds could be up to $5 million or more per year. In the light of almost certain increases in the cost of electrical power, and the possibility of a limitation on the total power available, we feel that the energy-saver aspects of this project could prove vital to the future ability of FERMILAB to support a strong and viable program.

The Subpanel endorses the view of the Weisskopf Subpanel that the Energy Doubler/Saver is a highly relevant intermediate step on the road to the long-term goal of very high energies at FERMILAB. The development of the technology of large-scale superconducting systems for the Energy Doubler/Saver will be an important asset in attaining this long-term goal. We note, however, that the character of both the material and the presentations describing the Energy Doubler/Saver proposal is of a different order of detail from that of the other proposals evaluated--and therefore our evaluation reflects this difference. At this stage the proposal resembles a large accelerator R&D project. However, even if only a portion of the hoped-for goals are achieved, the project would have significant payoffs in the energy achieved, the savings in electrical power, and the new technology developed. We have consequently recommended major funding for the Energy Doubler/Saver as an R&D project, with fabrication to follow in FY 1978 if adequate progress has been demonstrated.

Comments on Recommendation IV

The facility proposed by Cornell would permit the investigation of electron-positron colliding beam physics at energies higher than those presently available. The design uses a clever new method of injection from the existing 12 GeV synchrotron. We believe that the proposed device would produce important physics. It may provide the only possibility of having polarized electron-positron beams at 8 GeV per beam. However, the substantially higher maximum energy and the increased scope of the LBL/SLAC proposal make PEP the only acceptable choice as a single national electron-positron colliding beam facility.

The facility proposed by Cornell would provide a valuable addition to the U.S. High Energy Physics Program in a field which has proven rich beyond all expectation. Construction and exploitation of this facility would also insure continued vitality of a university based laboratory with a long history of accomplishments. For these reasons in a later section of this report, we recommend FY 1977 funding of the Cornell proposal under the Level D budget which supposes optimum exploitation of the promise of the field. However, within the constraints of Level A, under which we here make our recommendations, we do not recommend such funding.

It is true that compared to the overall guideline budget figures, the Cornell proposal is inexpensive. However, in the context of the long-range facility goals of the U.S. program, there is an urgent need to push on the three fronts of electron-positron colliding beams, proton-proton colliding beams, and very high energy fixed target accelerators, and therefore we do not consider the construction of a second electron-positron colliding beam facility as one of our highest priorities.

Section V

Impact of Alternative Funding Levels

Our recommendations and comments have assumed the constraints of the average annual funding Level A. In FY 1976 dollars these were $245 million for operations and equipment, and $50 million for new construction. In this section we consider the impact of the alternative funding Levels B, C, and D specified in the charge to the Subpanel. These have been taken to be (averaged over the next 10 years):

Level B:

$250 million total

Level C:

$190 million operations and equipment
20 million construction

Level D:

Closer to optimum[1]

Level B

The budget assumed by the Subpanel under Level B was a total of $250 million. Initially, the Subpanel considered the constraints imposed by the subdivision of that total into $215 million for operating and equipment funds and $35 million for construction.

There has been no major facility construction authorized for high energy physics since 1968. Since that time, important developments in physics and accelerator technology have opened three major paths for high energy physics to follow: electron storage rings, proton storage rings, and fixed target accelerators. We cannot afford scientifically to neglect or appreciably deemphasize any one of them. We view this three-pronged approach to the future as of primary importance to the well-being of high energy physics in the U.S. in the next decade. Thus, even within the more limited budget of Level B we see the need for construction funds at the level of $50 million per year. This would allow recommendations I, II, and III made earlier for the budget Level A to be carried out, as well as allow a start on the multi-TeV accelerator within the next 5 to 10 years, and we would recommend that here.

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[1] See footnote, p.4

We recognize that this would involve significant sacrifices within the present program. Recovery from present levels of utilization of existing facilities would not be possible. This plan also requires that, at the start-up time of a new major facility such as PEP or ISABELLE, there would necessarily be a substantial reduction in ongoing programs. We believe that these sacrifices are worth making to insure our primary objective--the continuation of a broad approach to the future of our field.

Level C

Level C corresponds to an operating and equipment budget of $190 million per year and an average construction budget of $20 million per year. The Subpanel believes that this budget level would be gravely damaging to the health of the field, and would seriously endanger both the short-term program and the long-term future. To understand the fiscal impact of this budget level, we remind the reader that during the 10 year base period 1965 to 1974 the average operating and equipment budget of high energy physics was $245 million; during the base period 1960 to 1974, the construction budget was $73 million. Evidently, budget Level C would severely limit the construction of new high energy research facilities, despite the great physics justification which already exists for them.

The need to pursue PEP as an immediate project remains foremost, and we would continue to recommend it. Under Level C constraints, however, it appears that the next major new facility after PEP would use up most or all of the construction funds for the rest of the decade. The need for both a high energy fixed target accelerator and for proton-proton colliding rings would still remain, and significant investments in accelerator development and superconducting technology should be made to that end, notwithstanding the severe limitations set by this funding level. We would therefore recommend substantial funds for both BNL and FERMILAB to pursue these goals.

We are aware that our recommendations at this level could only be carried out at great cost to the existing programs, most of which would have to be closed out as the new facilities came on to supply operating and equipment funds for the latter. We nevertheless believe that the health of the field requires that we move on with the new facilities to the extent possible.

Level D

The pursuit of understanding the ultimate structure of matter has led to the discovery of many new unexpected phenomena of which last year's discoveries are the latest examples. The U.S. High Energy Physics Program has produced a very large number of fundamental discoveries. One can therefore justify a budget which permits optimum utilization of existing capabilities and also permits the construction of new facilities as they are needed and can be exploited efficiently.

Within such a budget, we would change our recommendation IV for FY 1977 to permit authorization of the Cornell proposal for construction of an electron-positron colliding beam facility to operate with beams of 4 to 8 GeV at a design luminosity of $10^{32}cm^{-2}sec^{-1}$ at 8 GeV. This would permit a more rapid and comprehensive study of the 4 to 8 GeV energy region in the important area of electron-positron physics.

We also would recommend additional funds beyond the range of the Level A guidelines for the construction of new beam lines, experimental areas, and large detection systems to be used with the existing accelerators.

A Level D budget would expand to include operation funding for high energy laboratories and user groups beyond the confines of Level A in order to exploit the new facilities and productive ongoing programs. Such additional funding would yield a substantial return in important physics output.

This budget would also include funds for design studies and development programs with the aim of starting construction of a multi-TeV fixed target accelerator or multi-TeV proton-proton colliding beam facility in or about 1980. The need for such facilities has been discussed; the intervening years are likely to provide the information needed to make a choice and a suitable design.

We would also anticipate funding of the addition of an electron-proton capability at one of the new storage ring facilities in the early 1980's.

THE U. S. ATOMIC ENERGY COMMISSION
REPORT OF THE HIGH ENERGY PHYSICS ADVISORY PANEL
SUBPANEL ON THE HEALTH OF HIGH ENERGY PHYSICS

OCTOBER 1974

REPORT OF THE SUBPANEL
OF THE
HIGH ENERGY PHYSICS ADVISORY PANEL
ON THE
HEALTH OF HIGH ENERGY PHYSICS

Committee Members

David B. Cline	University of Wisconsin, Chairman
Albert G. Prodell	Brookhaven National Laboratory
James R. Sanford	Fermi National Accelerator Laboratory
Walter D. Wales	University of Pennsylvania

Introduction

The study of the health of any scientific field is subject to reasonable skepticism. Examining the health of the U.S. High Energy Physics Program, which only a few years ago was unexcelled in the world, is a particularly elusive and difficult undertaking.

The intellectual promise of this field lies in the fundamental quest toward an understanding of the ultimate structure of matter, which can be traced back to the early Greeks. It continues today with the present involvement of over 100 universities and colleges as well as five large accelerator laboratories in the U.S.

High Energy Physics represents a vital spearhead of physical science; it is the continuation of a frontier that started with Rutherford's discovery of atomic structure, continued with the insights into nuclear structure followed by the discovery of elementary particles, and is now penetrating into the structure of the particles themselves. Research in the field is relevant to the forces that hold the nucleus together, the discovery of fundamental new energy sources, the understanding of the development of the early universe, the evolution of stellar objects, the development of galaxies in the universe, and other cosmological problems. It has always attracted innovative minds because of the great challenges it offers in theory, experimentation, and instrumentation. In the pursuit of this work one faces technical and theoretical problems that go far beyond those which have been solved before. Furthermore, the advanced technology that has been employed in this field has led to many innovations of practical value, in such diverse areas as fast computers, superconducting devices, and sophisticated methods of data analysis. If vitality and vigor are drained from this field, U.S. science would lose one of its main driving forces.

In our assessment of the health of this field, we attempt to establish indicators that are as concrete as possible and to interpret these indicators, and then discuss the conclusions to which these interpretations appear to lead. The report is divided into four sections:

1. How Progress is Made in High Energy Physics.
2. Recent Trends in the Support and Operation of High Energy Physics Research.
3. Projection of Present Trends into the Future.
4. Elements of a Stable and Viable High Energy Physics Program in the U.S.

1. How Progress is Made in High Energy Physics

The history of physics, while not necessarily an accurate guide to the future, shows that progress in the understanding of physics generally comes about in a two-fold manner. On the one hand are the spectacular discoveries which are infrequently made, usually with the help of a new instrument or machine. Sometimes these discoveries seem to completely change the field overnight, as did the discovery of the neutron. On the other hand, necessary progress is also made by painstaking study and classification lasting over decades, such as the development of quantum mechanics through the study of atomic spectra. Both of these aspects of research are present in modern day high energy physics research and contribute to the health of the field. There is no doubt that the construction of major frontier particle accelerators has resulted in striking new discoveries, generally within a few years after the accelerator began operation. However, important work on the classification and understanding of the diverse particle spectrum continues even now on the lower energy accelerators. An example can be found in the study of the spectrum of meson elementary particles. In the early 1960's, mesons in addition to the known stable mesons (π, K, etc.) were easily discovered. These states fit very well into the then emerging SU(3) and Regge Pole theories of elementary particles. In turn, these theories predicted the existence of a large number of new states. In the ensuing years, the search for these additional states has progressed slowly but steadily. The SU(3) and Regge Pole theories cannot be fully put to the test of experiment until the meson spectrum is more fully explored. It is not possible to assess a priori whether such work will be equally important or perhaps more important to the ultimate understanding of the basic forces of nature than the spectacular discoveries made with newer accelerators. Both kinds of activity will probably be needed to form the ultimate explanation of these phenomena. To sustain the health of high energy physics, the innovation of new machines and also the long-term stability at lower energy machines at which the painstaking classification process can be carried out are needed.

A simplified picture of the activity in the field at present is that of two recently-completed frontier machines (FERMILAB and SPEAR) where important new areas of research are being pursued; and four older machines (Alternating Gradient Synchrotron, Zero Gradient Synchrotron, Stanford Linear Accelerator Center, and the Cornell Electron Accelerator) where important discoveries are being made and more detailed, systematic, and frequently very complex experiments are underway. Although all of these laboratories remain very productive, the fiscal limitations of recent years have severely strained their operations. In some cases future viability may already have been seriously compromised to maintain immediate productivity at reasonable levels. Of course, these stringencies at accelerator laboratories have led to a major reduction in the research opportunities for the university- and laboratory-based experimental groups.

2. Recent Trends in the Support and Operation of High Energy Physics Research

The real dollar support for high energy physics in the U.S. has declined over the past few years. Figure 1 shows the total funding, in terms of 1974 dollars, for the U.S. program. A more detailed analysis of the operating funds broken down by laboratory, shown in Figure 2, indicates a substantial decline in the funding for all existing facilities except FERMILAB. The effective dollar support for operation of the program has fallen by about 20 per cent since FY 1969. Three of the accelerators previously used by the Program (Cambridge Electron Accelerator, Princeton-Pennsylvania Accelerator, and California Institute of Technology

Synchrotron) have been shut down in the past five years, and a fourth (the Bevatron) is being phased out of particle physics research. At the same time, the accelerators remaining in the program are forced by funding limitations to operate much below the levels of optimum and efficient operation. A decreasing total amount of money is being stretched to cover the operation of FERMILAB and the other facilities. This state of affairs hampers the exploitation of the new accelerator at FERMILAB and severely restricts research at the other accelerators. Many excellent research projects are indefinitely postponed or must be carried out with insufficient means.

In Europe, in contrast to the U.S., the effective funding for high energy physics has steadily increased during the past decade. Figure 3 shows the relative trends of funding in the U.S. and Europe. The increasing productivity, and the availability of many unique sophisticated facilities in Western Europe, have made the high energy physicists in the U.S. even more sensitive to the problems of the program here. During FY 1974, a single laboratory in Europe (CERN) received more support than the entire program in this country. In the past few years, several important discoveries have been made first in the European laboratories reversing a trend of the previous two decades. While important discoveries continue in the United States, the leadership of the U.S. program in this field is no longer unchallenged and many physicists fear that the leadership will move to Europe.

Due to the reduced funding, U.S. accelerators operate at levels as low as 40 per cent of capacity. The interval of time between when a typical experiment is approved and when the experiment is actually set up at the accelerator now is about two years. This long delay serves as a deterrent to originality in the U.S. program. One of the most significant effects of the low funding level has been the inhibition of daring new experiments.

A direct result of the decreased funding in the program is evident in the reduction of the total personnel as shown in Figure 4. This total has dropped from about 8500 in FY 1969 to about 6300 in FY 1975. Such a loss of experienced trained personnel jeopardizes the future health of the Program. Professional personnel, as shown in Figure 5, have declined in a similar fashion. These people are the technicians and engineers who have developed expertise in the sophisticated technical needs of high energy physics experiments. They cannot be replaced by inexperienced trainees, and represent a clear loss of talent to the field.

The steep drop in the enrollment of graduate students shown in Figure 5 is a partial result of the cuts in funding. The number of graduate students in the AEC High Energy Physics Program has dropped precipitously, from over 800 in 1968 to less than 400 in 1974, as the consequences of continuing funding reductions have become apparent to prospective high energy physicists. The very long time between the conception of an experiment and its ultimate completion discourages bright, ambitious young people. The lack of opportunity for young physicists to carry out an experiment of their own conception is particularly disheartening. There is now a shortage of excellent graduate students and young research associates to participate in the ongoing experiments.

The total number of operating shifts at AEC high energy physics accelerators since FY 1967 is shown in Figure 6. In spite of the fact that CEA and PPA have been closed and the Bevatron is being phased out of high energy physics, the accelerator shifts at the AGS, SLAC, and the ZGS have declined. Even when FERMILAB is added, the total number of shifts is considerably less now than in FY 1967.

An indicator which may be more directly related to research activity is the total number of research hours, which is effectively the number of hours of accelerator operation multiplied by the number of experiments running simultaneously. Figure 7 shows that the total number of research hours in the AEC program has decreased since reaching a peak in FY 1970. However, the number of research hours at the present four AEC-supported high energy laboratories has actually increased somewhat, both because of the emerging operation of FERMILAB, and because past capital investment has permitted an increase in the average number of simultaneous experiments thereby allowing a somewhat increased productivity of older accelerators even in the face of decreasing funding. However, the program has probably done as much as possible to increase multiplicity, and future reductions in accelerator shifts will also be reflected in the research hours.

An indication of the reduced output of the U.S. High Energy Physics Program in the past few years can be found in the number of experimental papers published in Physical Review Letters, the primary U.S. journal for topical physics results. As shown in Figure 8, U.S. publications in this journal declined while European publications in Physics Letters, the corresponding European journal, increased.

The number of experiments for which the machine running time has been completed as a function of time is shown in Figure 9. As expected, while there is a healthy increase at FERMILAB, the AGS, SLAC, and the ZGS have shown an overall decrease. The experiments at the CEA and the PPA have now completely stopped, while the number of experiments at the Bevatron has decreased and will soon cease. The total number of experiments completed per year has dropped greatly since 1970.

On the whole, the output of the overall U.S. High Energy Physics Program shows a significant drop in the past few years. The continuing vitality of the program is due to the turning on of FERMILAB and SPEAR and to the careful management of the other laboratories in the face of severe budget problems. While each indicator presented does not by itself support definitive conclusions, all of these indicators, when taken together, appear to give strong evidence that the output of the program has declined significantly in recent years.

3. Projection of Present Trends into the Future

The simplest approach to projection of present operating funds would be a straight line fit to the data shown in Figure 2 from FY 1969 through FY 1975. Since the extrapolation of this line into the future would indicate complete elimination of funding in about 20 years, a somewhat more optimistic assumption, that the future total operating funding would be no lower, in FY 1974 dollars, than the level of approximately $142 million for that year, was made. We also projected the FY 1975 construction level (almost nothing) and a growing program (to the operating level of $45 million) at FERMILAB within this level of spending.

The total personnel in the program would level off at about the present level. The funding for FERMILAB, in this case, would be about 25 per cent below the level that is necessary for optimum exploitation. The program there would expand somewhat above the present levels, while the programs at the other laboratories would continue to be curtailed to release funding for even these modest increases at FERMILAB. It seems likely that at least one laboratory or major segments of several laboratories would have to be shut down in the next few years. The basic hadron research at lower energies would be reduced by a large factor. The continued lack of adequate funding for accelerator improvement projects, which has been of crucial importance in the maintenance of productivity at the laboratories during the past five years of declining operating funding, would accelerate the obsolescence of all the laboratories in the program, including FERMILAB.

An even more ominous threat to the health of the U.S. High Energy Physics Program would be the continued deferral of capabilities for frontier research such as PEP at SLAC/LBL, ISABELLE at BNL, or higher energy proton capability at FERMILAB. The vigor of the field during the past decade has been based on the mutually beneficial interaction between exploratory work at the very highest energies and painstaking and deliberate systematic study at lower energies. The abandonment of new initiatives on the energy frontier, coupled with the atrophy of efforts at lower energy would inevitably lead to vitiation of the entire program.

4. Elements of a Stable and Viable High Energy Physics Program in the U.S.

The above discussion shows clearly that present decreasing funding trends in high energy physics, if continued, will lead to severe constraints in the program in the near future and eventual erosion of the program. A program in high energy physics that exploits current research opportunities while providing some new facilities is the minimum requirement to maintain the health of the field.

Exploitation consists of doing experiments at existing accelerators taking advantage of new research results and improved technology. To do this even in a no-growth environment requires the availability of constant operating support in real dollars with additional funds for innovations and development. These additional funds would be specifically intended to support improvements in accelerator usage, secondary beam transport, and detector equipment. These improvements are necessary if the productivity in high energy physics is to be maintained as experiments become increasingly more difficult and complex. The additional funds would represent equipment and accelerator funding support that is 20 per cent of the inflation-compensated operating budget.

The new capabilities envisioned would be provided by authorization of new construction such as e-e, e-p, or p-p storage rings, or higher energy fixed-target facilities. Facilities such as those suggested in the recent report of the Subpanel on New Facilities would give vitality to the field in the frontier area.

In time the operating support for such new projects would have to be found. If incremental operating funds are not available for the new facilities, although that would obviously be most desirable, other solutions must be contemplated that will make it possible to utilize these new facilities while maintaining the health of the field. Perhaps the financial support for the ongoing research program at the parent laboratory would need to be transferred to the new facility. If that is not sufficient, then research elsewhere in the High Energy Physics Program would need to be curtailed. It is recommended that this not be done entirely at the expense of the lower energy program. The importance of the lower energy region in supplying a foundation for the field is hereby stressed. The health of high energy physics will be sustained only if the U.S. has facilities that can be operated for systematic research as well as frontier experiments.

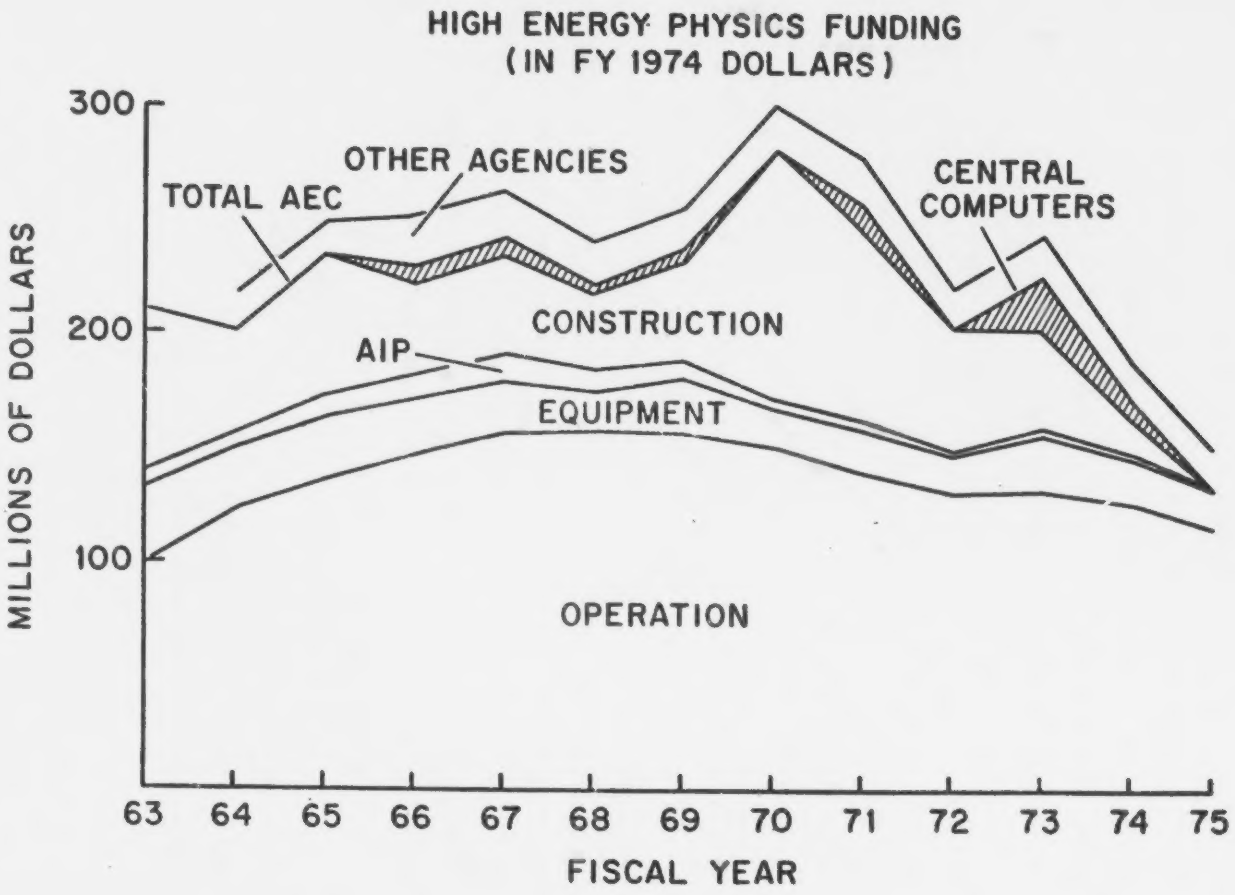
HIGH ENERGY PHYSICS FUNDING
(IN FY 1974 DOLLARS)
MILLIONS OF DOLLARS
300
200
100
TOTAL AEC
OTHER AGENCIES
CENTRAL COMPUTERS
CONSTRUCTION
AIP
EQUIPMENT
OPERATION
63
64
65
66
67
68
69
70
71
72
73
74
75
FISCAL YEAR

Figure 1

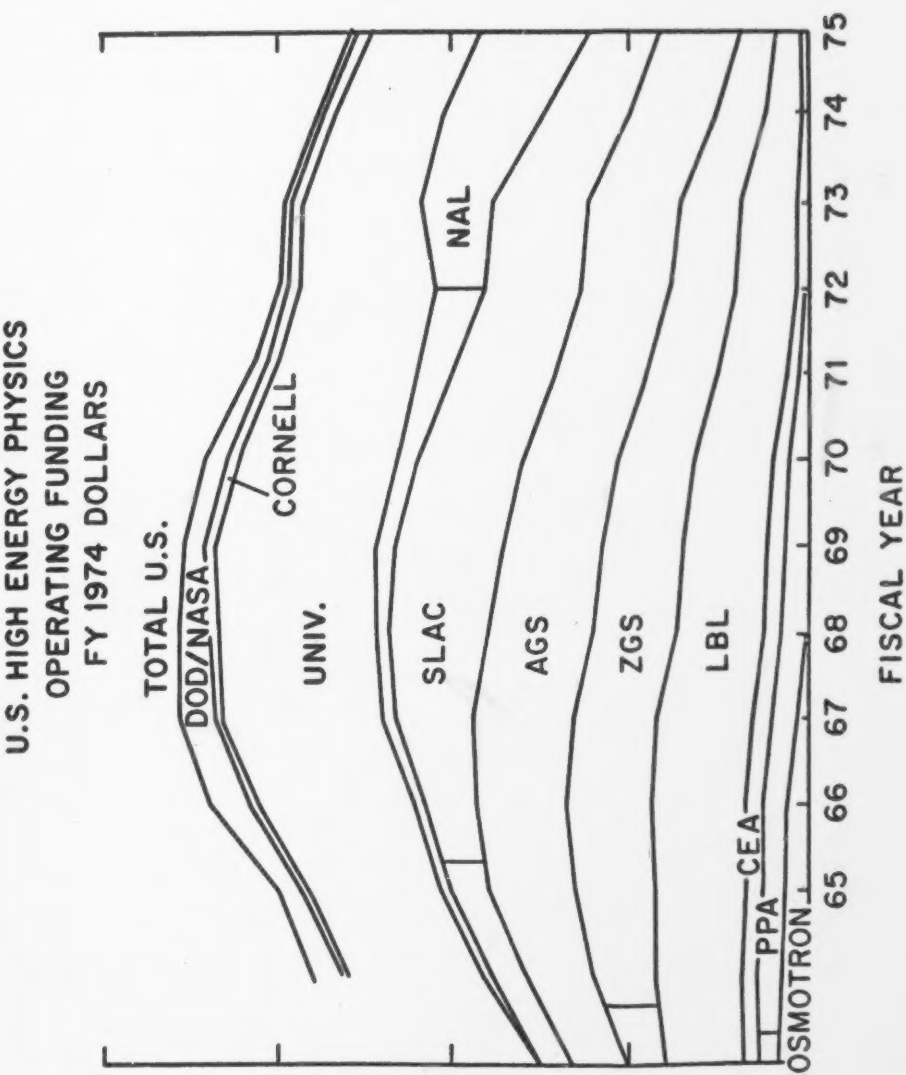
U.S. HIGH ENERGY PHYSICS
OPERATING FUNDING
FY 1974 DOLLARS
TOTAL U.S.
DOD/NASA
CORNELL
UNIV.
SLAC
NAL
AGS
ZGS
LBL
CEA
PPA
COSMOTRON
MILLIONS OF FY 1974 DOLLARS
200
150
100
50
FISCAL YEAR
65
66
67
68
69
70
71
72
73
74
75

Figure 2

TOTAL GOVERNMENT SUPPORT FOR HIGH ENERGY PHYSICS

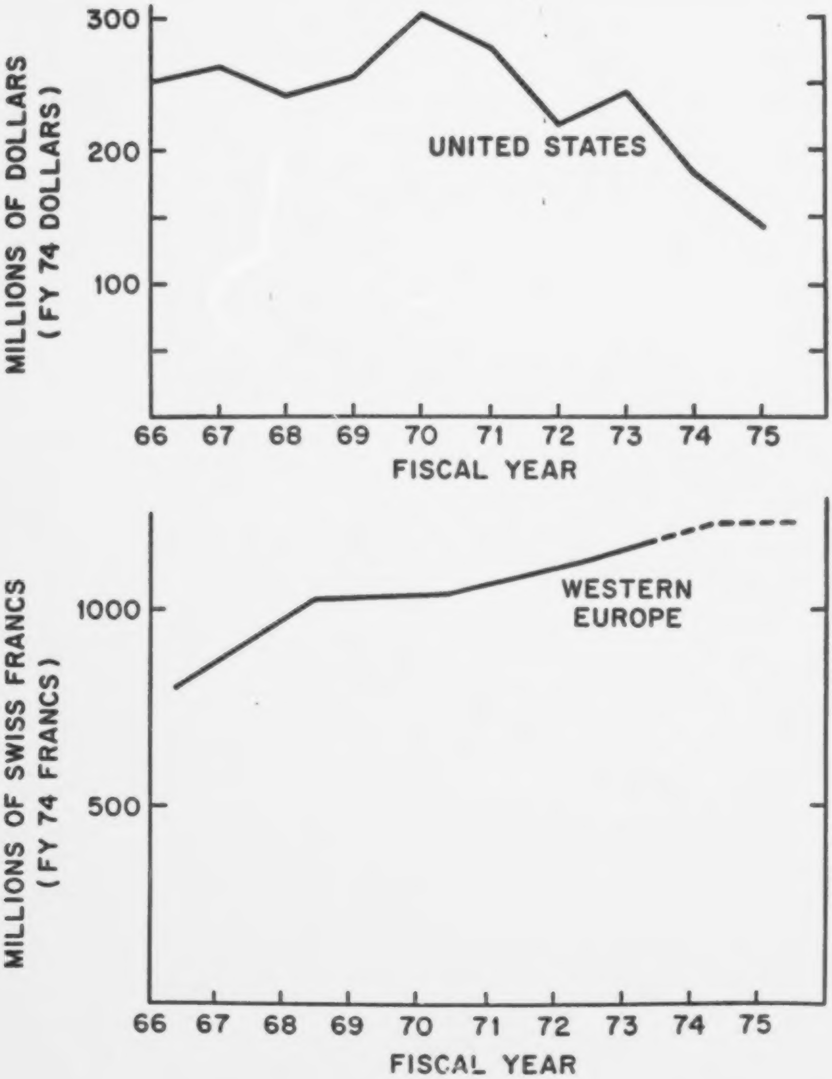

Figure 3

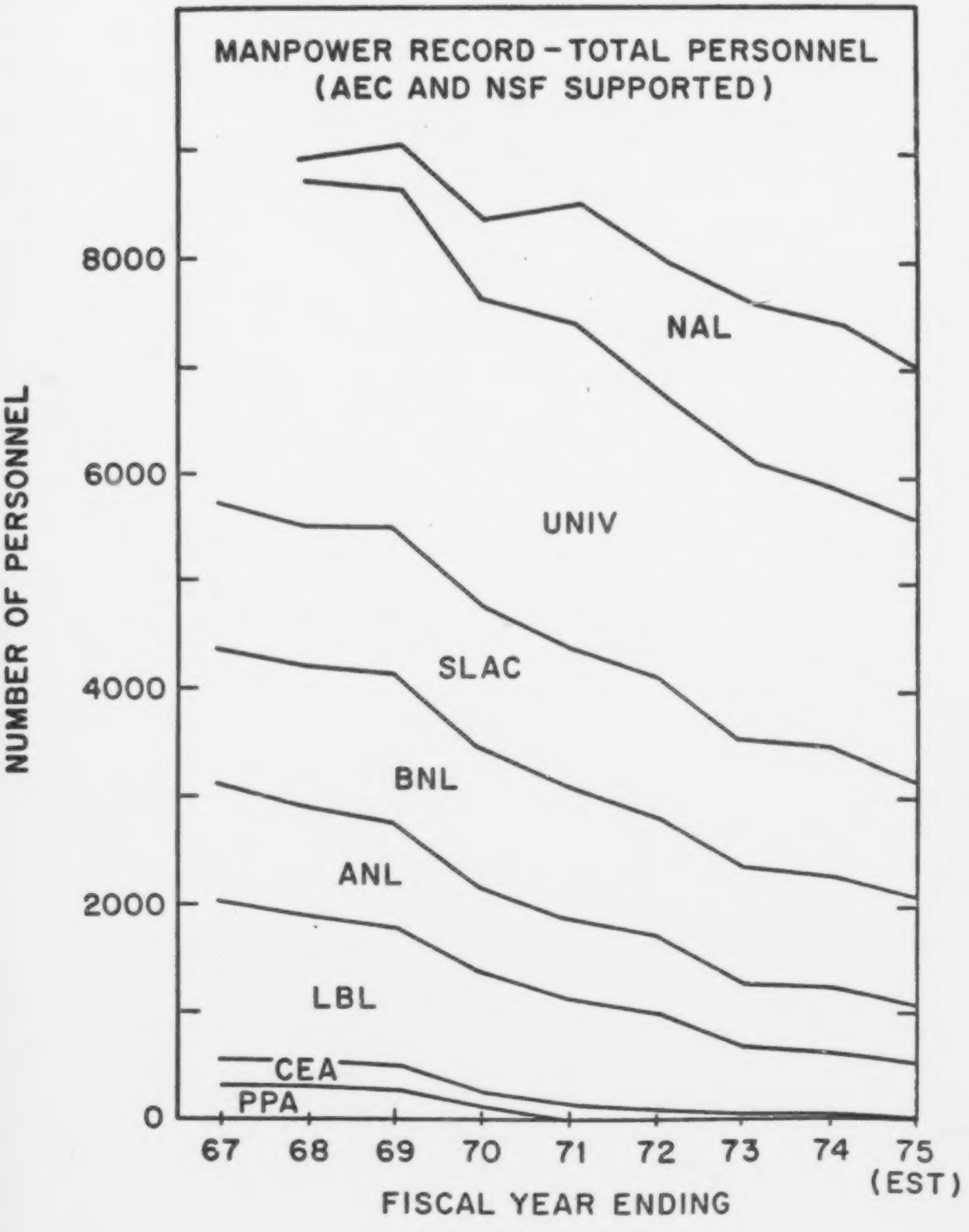
MANPOWER RECORD - TOTAL PERSONNEL
(AEC AND NSF SUPPORTED)
NUMBER OF PERSONNEL
0
2000
4000
6000
8000
NAL
UNIV
SLAC
BNL
ANL
LBL
CEA
PPA
67
68
69
70
71
72
73
74
75
(EST)
FISCAL YEAR ENDING

Figure 4

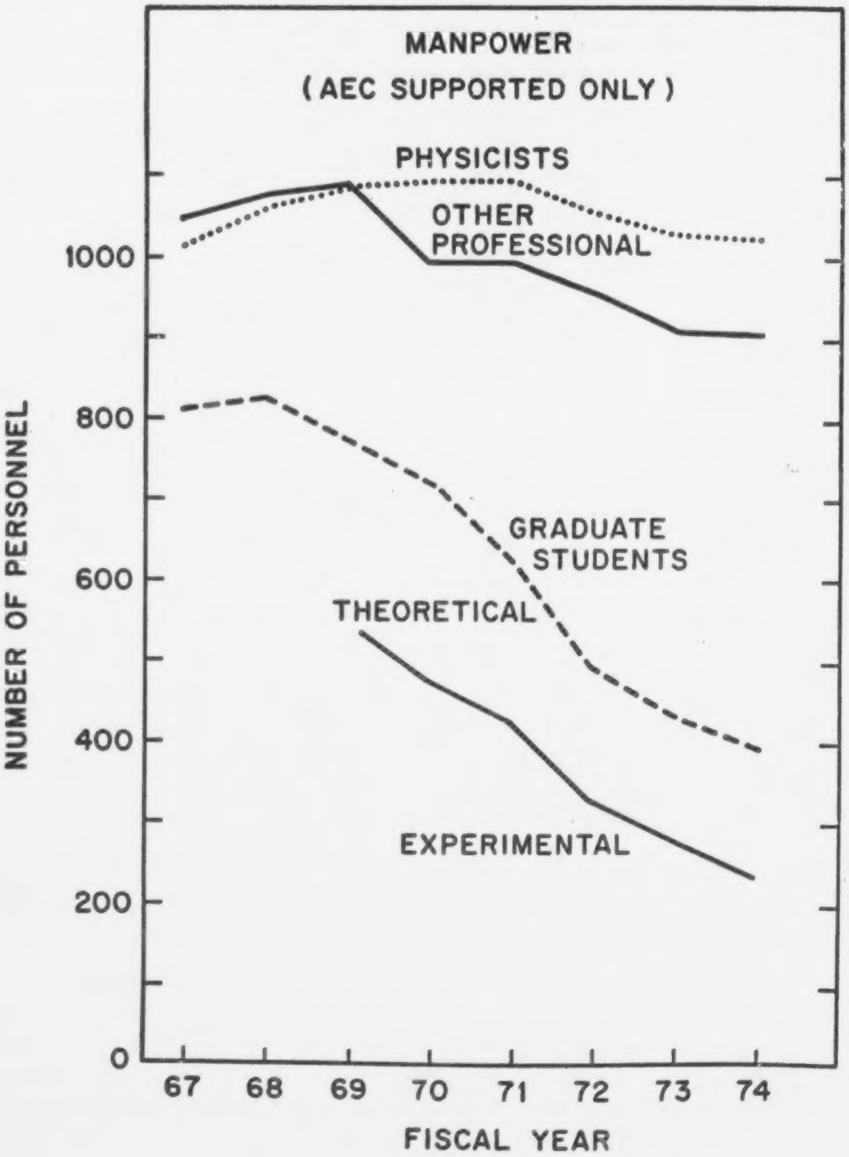
MANPOWER
(AEC SUPPORTED ONLY)
PHYSICISTS
OTHER PROFESSIONAL
GRADUATE STUDENTS
THEORETICAL
EXPERIMENTAL
NUMBER OF PERSONNEL
0
200
400
600
800
1000
67
68
69
70
71
72
73
74
FISCAL YEAR

Figure 5

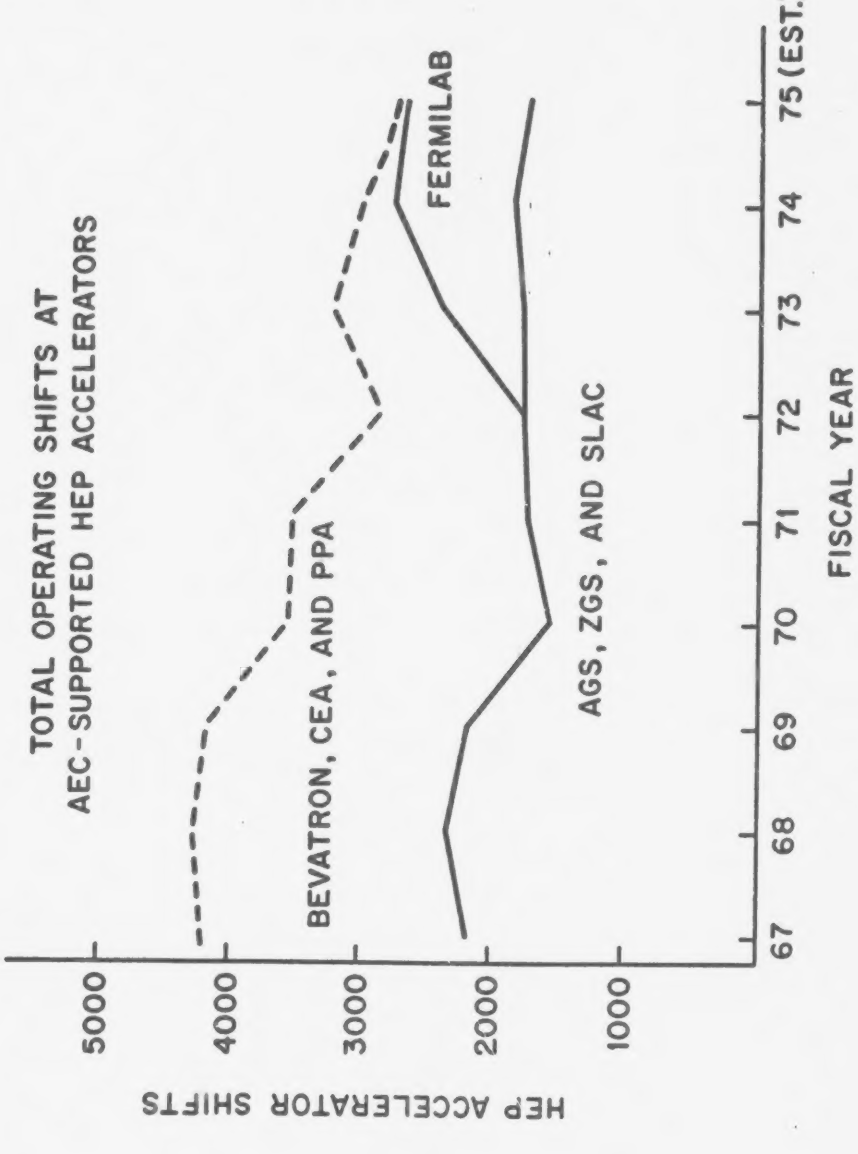
TOTAL OPERATING SHIFTS AT
AEC-SUPPORTED HEP ACCELERATORS
HEP ACCELERATOR SHIFTS
5000
4000
3000
2000
1000
BEVATRON, CEA, AND PPA
FERMILAB
AGS, ZGS, AND SLAC
67
68
69
70
71
72
73
74
75 (EST.)
FISCAL YEAR

Figure 6

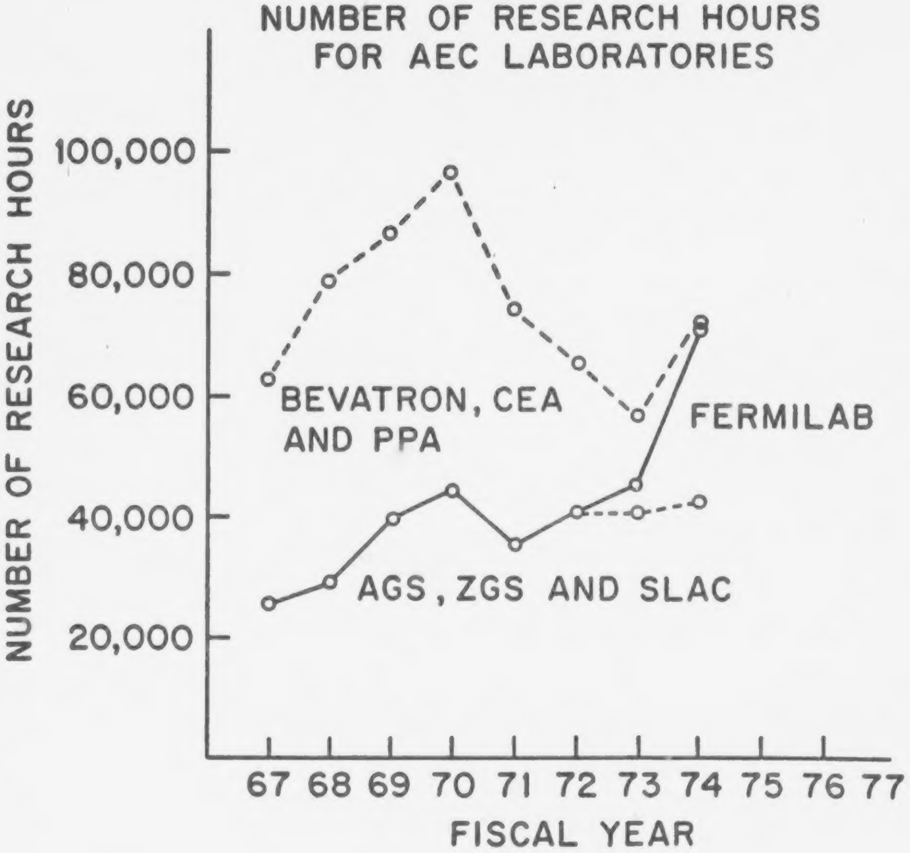
NUMBER OF RESEARCH HOURS
FOR AEC LABORATORIES
NUMBER OF RESEARCH HOURS
100,000
80,000
60,000
40,000
20,000
BEVATRON, CEA
AND PPA
FERMILAB
AGS, ZGS AND SLAC
67
68
69
70
71
72
73
74
75
76
77
FISCAL YEAR

Figure 7

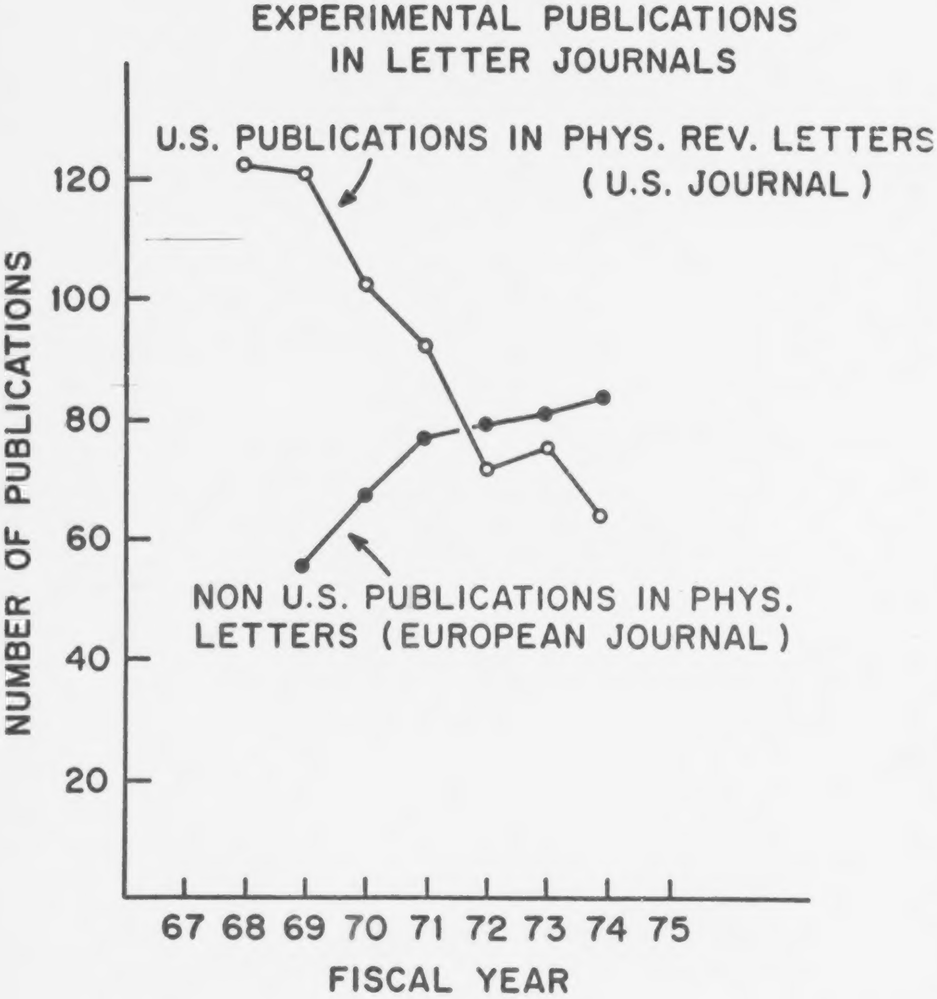
EXPERIMENTAL PUBLICATIONS IN LETTER JOURNALS
U.S. PUBLICATIONS IN PHYS. REV. LETTERS (U.S. JOURNAL)
NON U.S. PUBLICATIONS IN PHYS. LETTERS (EUROPEAN JOURNAL)
NUMBER OF PUBLICATIONS
120
100
80
60
40
20
67
68
69
70
71
72
73
74
75
FISCAL YEAR

Figure 8

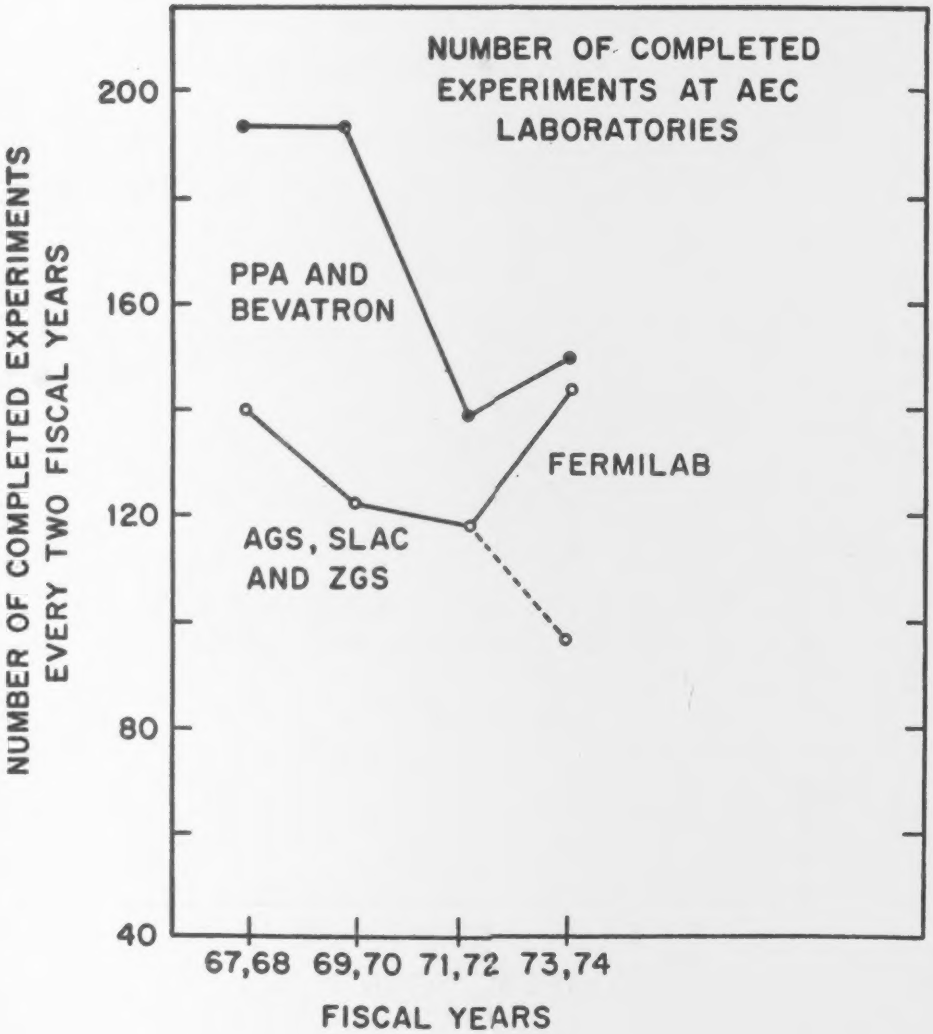
NUMBER OF COMPLETED EXPERIMENTS AT AEC LABORATORIES
NUMBER OF COMPLETED EXPERIMENTS EVERY TWO FISCAL YEARS
200
160
120
80
40
PPA AND BEVATRON
FERMILAB
AGS, SLAC AND ZGS
67,68
69,70
71,72
73,74
FISCAL YEARS

Figure 9

THE U. S. ATOMIC ENERGY COMMISSION
REPORT OF THE HIGH ENERGY PHYSICS ADVISORY PANEL
SUBPANEL ON NEW FACILITIES

JULY 30, 1974

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

July 30, 1974

Dr. John M. Teem, Director
Division of Physical Research
U. S. Atomic Energy Commission
Washington, D. C. 20545

Dear John:

HEPAP has received and reviewed the Report of its Subpanel on New Facilities. The Subpanel's charge was to consider the future needs for high energy facilities and determine the next major steps required in this field over the coming decade; and specifically to determine what facilities, if any, should be started in FY1976.

High energy physicists throughout the world agree that, to obtain a deep understanding of the laws of behavior of matter on the submicroscopic scale, one must explore the relations between the structure and the interactions of hadrons, and between the weak, the electromagnetic, and the strong force laws. Results of recent research strongly suggest that we are on the verge of a new and very basic and important synthesis that needs to be probed and tested by the facilities recommended by the Subpanel.

Major progress during recent years toward understanding the most basic force laws and elementary structures in nature has shown that the information gained from using lepton and hadron probes is becoming increasingly interdependent. Consequently, the Subpanel concluded that increasing the energy of both lepton and hadron probes is necessary. The HEPAP concurs in this conclusion and agrees that it is essential to push the energy frontier forward with positron-electron colliding beams, with proton-proton colliding beams, and with a fixed target proton accelerator of at least 1000 GeV.

Positron and electron beams colliding in storage rings would provide the source of an initial matter/energy state of particular simplicity, uncomplicated by strong interactions, in a new energy range where the validity of the fundamental laws of electricity and magnetism, the properties of the weak interactions, and the forms of matter into which pure electrodynamic energy materializes can be uniquely explored for the first time. Proton-proton colliding beams on the other hand provide the highest effective energies - energies which would be prohibitively expensive if provided by fixed target machines or electron colliding beams. With these colliding proton beams it is possible to explore beyond thresholds for creating new and theoretically expected particles, and to probe the basic properties and changing character of the strong nuclear forces at smaller distances. In contrast a high energy proton accelerator whose beam impacts against fixed targets in the laboratory, while not attaining effective energies as high as colliding beam machines, makes it possible to generate intense high energy secondary beams of diverse particles, including for example beams of high energy neutrinos for studying the fundamental weak β decay forces in nature.

The following constraints were assumed by the Subpanel in making its study:

1) The budget for the high energy physics program will be near a level of roughly $200 million in FY1974 dollars for operations and equipment. This is about the average of such spending (in FY1974 dollars) in the U.S. over the past ten years. The constraint of a near constant level of funding for operations implies a constant manpower level with experiments at the new highest energy regions supplanting some of the work at older facilities.

2) For a science at the frontiers of the unknown, it is important to devote a substantial part of the program effort to the provision of new and innovative facilities. Past experience in high energy physics shows that an average expenditure for construction of such facilities of 20 to 25% of the total funding supporting the field provides a healthy balance between exploitation of existing facilities and developments for the future.

   The average funding for construction in high energy physics over the past fifteen years (FY1960 through FY1974) has been about $60M per year (in FY1974 dollars). The Subpanel guidance from the AEC was to consider an average of about $40M per year over the next ten-year period as a reasonably constrained level for new facility construction for the program.

Mindful of the budgetary constraints and the present state of technology the Subpanel recommendations, focussing on FY1976, are:

1) Authorization in FY1976 of the LBL-SLAC proposal for the construction at SLAC of a 15 GeV electron-positron colliding beam facility ("PEP") with a design luminosity of $10^{32}cm^{-2}sec^{-1}$.

2) Providing funds in FY1976 of $3-4M for BNL to complete fabrication of prototypes of superconducting magnets, with the aim of making a request for early construction at BNL of a proton-proton colliding beam facility ("ISABELLE") with a design luminosity of $10^{33}cm^{-2}sec^{-1}$ and a beam energy of at least 200 GeV.

3) Funds be provided to support an accelerator development program at FERMILAB directed toward the long-term goal of fixed target and/or colliding beam systems in the region of 1000 GeV and above.

HEPAP strongly endorses all of the above recommendations. It notes that these new capabilities, each of which would provide a substantial increase in energy over that currently available, are achieved by making major and direct use of existing high energy physics facilities. It also believes that dispersal of facilities throughout the country, which would result from the recommendations, assists in providing the diversity of physics, style, and intellectual input which is desirable for a fruitful scientific endeavor.

Sincerely yours,

Sid

Sidney D. Drell, Chairman
High Energy Physics Advisory Panel
to the Atomic Energy Commission

SDD:br

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

June 15, 1974

Dr. Sidney Drell
Chairman,
High Energy Physics Advisory Panel
c/o Stanford Linear Accelerator Center
P. O. Box 4349
Stanford, California 94305

Dear Dr. Drell:

The HEPAP Subpanel on New Facilities has deliberated for a week at the National Academy's Houston House in Woods Hole and has written a report containing specific recommendations and general comments in regard to the future requirements of particle physics. In FY 76 eight years will have passed since the last major authorization was granted for a new facility in high energy physics. Furthermore, the U.S. do not possess any proton colliding beam facilities whatsoever. In this time interval many new discoveries were made which point to the richness of the field. They raise the expectations of new and unexpected phenomena to be discovered and of new insights into the laws of physics to be gained at higher energy.

I hope that the Panel considers our conclusions and transmits the report to the A.E.C. as soon as possible, in view of the importance of this report for the FY 76 decisions.

With best wishes,

Yours,

Victor F Weisskopf

V. F. Weisskopf

VFW:dle

Introduction

High energy physics is directed toward an understanding of the ultimate structure of matter and seeks the answers to the questions that are basic to science and its applications, such as the nature of the force that holds the atomic nucleus together, the nature of radioactivity, and the reasons for the observed strength of the electric forces on which all chemistry is based. High energy physics research has led to the discovery of many new and unexpected natural phenomena, such as new forms of matter including antimatter, its creation and annihilation, many new particles and their transformation processes. If the experience of the past can be used to forecast the future, then these discoveries and the technical innovations that led to them, will not remain without practical applications within a few decades.

In the last few years a great variety of new exciting results and ideas have come up in high energy physics that make it imperative to extend the exploration to new frontiers. It is now clear that much greater insight into the details of the structure of elementary particles is to be gained by making observations of elementary particle collisions at still higher energies. A number of studies and proposals for major new facilities have been made, including positron-electron (e^+e^-), electron-proton (ep), and proton-proton (pp) colliding beam devices as well as fixed target accelerators, all aiming to extract new information, seek out new phenomena and test promising theoretical insights. With these in mind, the Atomic Energy Commission has requested a Subpanel of the High Energy Physics Advisory Panel (HEPAP) to consider the future needs for high energy facilities and to determine the next major steps required in this field over the next decade.

The HEPAP Subpanel on New Facilities was formed in May, 1974 and decided to concentrate its efforts in a week-long meeting with the goal of making a preliminary report by the end of June, 1974 in order to be useful for the FY 1976 budget cycle.

The HEPAP Subpanel on New Facilities chaired by V. F. Weisskopf had the following members, and consultants and visitors:

Members:

B. C. Barish (Cal. Inst. Tech.)
V. Fitch (Princeton)
H. Frisch (Chicago)
M. Hine (CERN)
K. Johnsen (CERN)
D. Keefe (LBL)
T. D. Lee (Columbia
J. E. Leiss (NBS)
F. E. Low (MIT)
B. D. McDaniel (Cornell)
Y. Nambu (Chicago)
K. Strauch (Harvard)
M. Strovink (LBL)
V. L. Telegdi (Chicago)
V. F. Weisskopf, Chairman (MIT)
B. Hildebrand, Exec. Secy. (AEC)

In addition the following persons attended certain sessions:

Consultants:

M. Q. Barton (BNL)
S. J. Brodsky (SLAC)
D. A. Edwards (FERMILAB)
L. J. Laslett (LBL)
L. M. Lederman (Columbia)
J. R. Purcell (ANL)
J. Rees (SLAC)
W. B. Sampson (BNL)

Observers or Visiting Participants:

S. D. Drell, Chairman, HEPAP (SLAC)
G. K. Oertel (NSF/Science Technology Policy Office)
B. Richter (SLAC)
R. R. Wilson (FERMILAB)
E. Goldwasser (FERMILAB)
N. Ramsey (Harvard)
G. Vineyard (BNL)
R. Rau (BNL)
A. Abashian (NSF)
H. L. Kinney (AEC)
W. A. Wallenmeyer (AEC)
J. M. Teem (AEC)

The Subpanel considered two detailed proposals: PEP electron-positron colliding beams (15 GeV each) and ISABELLE proton-proton colliding beams (200 GeV each). It also discussed the Fermi National Accelerator Laboratory (FERMILAB) energy doubler project (~ 1000 GeV), steps and ideas toward higher energies at FERMILAB such as proton-proton colliding beams, electron-proton colliding beam options on the e^+e^- and pp devices, and a several-thousand GeV fixed-target accelerator.

The two proposals were considered as to their technical feasibility. All devices were discussed as to their potential importance for new discoveries in physics and in the context of both U.S. and European plans in this field.

The Subpanel was assisted by thoughtful responses to V. F. Weisskopf on his request to members of the Division of Particles and Fields of the American Physical Society for their views on future planning in this field. Over 100 individuals responded to this request.

The report is structured in the following way:

Section I - The Status and Promise of Particle Physics
Section II - The Means Required for Further Advances
Section III - Technical Considerations
Section IV - Recommendations and Comments

The Recommendations of the Subpanel are on page 17.

Section I

The Status and Promise of Particle Physics

Physics advances by finding connections between apparently diverse phenomena. Over one hundred years ago, Maxwell showed that electricity, magnetism and light were but different aspects of the same phenomena. The mathematical laws written down by Maxwell have survived the revolutions of quantum theory and special relativity, and are valid from astronomical down to subnuclear distances! By the first third of this Century these laws had succeeded, in the words of Dirac, in "accounting for the greater part of physics and all of chemistry," that is, for the structure of ordinary matter.

Today we may be on the verge of a similar, and perhaps even deeper, synthesis. To explain this synthesis, we note that on the basis of all experimental facts to date, elementary particles naturally divide into three categories: (1) the proton; (2) leptons [negative and positive electrons, heavy electrons (muons), and uncharged, massless neutrinos]; and (3) hadrons (protons, neutrons, hyperons, mesons). These categories are formed on the basis of the three very different kinds of forces through which particles interact with each other; according to their strength, we distinguish between strong, electromagnetic and weak interactions. These strengths are, very roughly, in the ratio of 1 to 1/100 to 1/1,000,000. Further, only the hadrons interact strongly (in fact, that is how they are defined); particles carrying electric charge and also the photon have electromagnetic interactions, and all particles except the photon have weak interactions.

This classification has revealed itself from a half century of experimental effort, spanning a factor of more than 100,000 in particle energy, and until now has shown no sign of breaking down. Today, we have a means of reaching a radically new domain of energy through the use of storage rings and colliding beams. Whereas in all other types of accelerators the effective energy E_{cm} (center-of-mass energy) grows only with the square root of the beam energy, in a colliding beam system the effective energy is twice the beam energy.

The hope for synthesis rests primarily on the fact that electromagnetic cross sections tend to remain constant, or decrease with the energy of the incident beam, whereas weak interactions increase with energy. Assuming that this experimentally observed trend continues*, the two must become comparable at an effective energy given roughly by $E_c \sim 30$ GeV.

The electromagnetic interaction is mediated by the photon. The unification with the weak interaction implies that it too should be mediated by a carrier (which is called the W particle). The mass of the W is expected to be greater than or in the range of the characteristic energy, E_c. Experiments with leptons of approximately this energy are bound to reveal some new insight into electromagnetic and weak interactions and there is promise that the results may show the basic unity of the two. Although some important features of these phenomena can be determined by experiments in the multi-GeV region, the only fully convincing verification of the ideas involved will require actually producing the new W particles. In addition, there are suggestions of the existence of new leptons. The masses of these leptons, if any, cannot be predicted theoretically, and therefore their discovery would open up new perspectives.

Although the interaction of hadrons is strong and complicated, many remarkable simplicities have been observed in their structure in experiments carried out over the past decade. In particular, the hadrons behave in many ways as if they were composed of subunits ("quarks") carrying one-third and two-thirds of the charge of the electron, which until now has been considered a fundamental unit. This simple picture accounts qualitatively, and in some cases almost quantitatively, for the existence and properties of the observed hadron states, for the energy dependence of certain classes of hadron reactions, and for the inelastic scattering of leptons by protons.

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*If the trend were not to continue, then that would in itself be a remarkable discovery: either Maxwell's electrodynamics would finally have failed or the weak interaction would reveal a new structure.

The model encounters difficulties in two respects. First free quarks have not so far been produced in high energy collisions. Second, the model appears to give an incorrect description of the production of hadrons by the annihilation of electrons and positrons. It predicts a cross section that falls as the energy increases; instead, the cross section as observed in the Cambridge Electron Accelerator and Stanford Linear Accelerator Center e^+e^- colliding beams remains constant up to an effective energy of 5 GeV. This unexpected constant cross section is not understood. It may represent a general breakdown of the quark picture, or, speculatively, it may be a threshold effect reflecting the existence of entirely new kinds of particles. In fact, certain properties of the weak interactions seem to require such new hadrons, with multi-GeV masses. It is of extreme interest to know whether the cross section remains constant up to considerably higher energies, since, if this were so, large modifications of purely electrodynamic processes must occur.

Until recently, the available data had led to the belief that all hadron cross sections reach a constant value at high enough energy. Now on the basis of experiments carried out at Serpukhov, CERN, and FERMILAB, it is clear that this is not so; the cross section as a function of energy is not constant, but rises gently. Thus, we have not yet reached a high energy limit, and a new region is opening up for exploration of strong interactions.

An effective way to probe the inner structure of hadrons is to use "hard" or "head-on" collisions, where one hadron is appreciably deflected by another particle, either hadron or lepton. As the energy and deflection angle increase, the hadron structure is probed deeper and deeper. Theory indicates that the essence of the hadron structure is most readily explored in this region. Recent experiments at the CERN Intersecting Storage Rings (ISR) and FERMILAB show that as the collisions become "deeper", the strong interactions become significantly weaker. A second characteristic energy is defined as the one for which the weak interaction becomes comparable to the strong interactions in deep collisions. This energy is in the region of several hundred GeV. At such energies and at large angles, even if the expected merging of the weak and electromagnetic interaction has not yet occurred, weak processes will compete with strong ones and may reveal some deeper relationship.

In summary, the following are examples of the discoveries awaiting us in the next energy range: the possible theoretical synthesis of weak, electromagnetic and perhaps even strong interactions as they become comparable in strength; production of W particles, excited leptons, and other exotic new particles; detailed exploration of the inner structure of hadrons; and perhaps the discovery of a new sub-nuclear world with totally unexpected features. In the past, the opening up of a new energy region always has shown that nature is much richer than we had predicted from the results at lower energies. It is difficult to express in guarded terms the enthusiasm generated in the high energy physics community by the prospect of such major breakthroughs.

Section II

The Means Required for Further Advances

As has been emphasized in the previous section, the information gained from using lepton and hadron probes is becoming increasingly interdependent. Consequently, increasing the energy of a single type of facility is not adequate. The energy frontier should be pushed forward with positron-electron colliding beams, with proton-proton colliding beams, and with a fixed target proton accelerator of at least 1000 GeV.

Each of these new facilities should provide a substantial increase in energy over that currently available (see Table I). The positron-electron beams colliding in storage rings provide the source of an initial state of particular simplicity, uncomplicated by strong interactions, in an energy range where one anticipates the observation of effects involving both the weak and the electromagnetic interactions. The proton-proton colliding beams provide the highest effective energies - energies which would be prohibitively expensive if provided by fixed-target machines. However, the variety of collisions that can be studied by these means is restricted to primary interactions of the accelerated particles.

In contrast, a fixed-target proton accelerator, while not attaining the high effective energies of colliding-beam machines, has the immense virtue of a large interaction rate since the fixed target consists of matter rather than of a diffuse beam. Consequently these accelerators produce intense secondary beams of a wide variety of particles whose energies approach the primary energy of the machine.

Table I

The Present Highest Energy Facilities

Name	Location	Type	Energy (GeV)
SPEAR I	US (SLAC)	e^+e^-	2.5x2.5
SPEAR II	US (SLAC)	e^+e^-	4.5x4.5 (soon)
DORIS	Germany (DESY)	e^+e^-	3.0x3.0 4.5x4.5 (soon)
ISR	Europe (CERN)	pp	30x30
FERMILAB	US	fixed target p	400
SPS	Europe (CERN)	fixed target p	400 (soon)

As a specific example of the interrelation between apparently diverse processes, we point to the production of hadrons in the SLAC electron-positron storage rings, the exploration of the structure of hadrons at SLAC using electron beams and at FERMILAB using muon and neutrino beams, and the anticipated production of leptons in proton-proton collisions at the CERN ISR. As we progress beyond the energy frontier to regions where the different types of interactions become comparable in strength, the richness of this interrelationship should grow.

In high energy physics, as elsewhere, rapidly evolving technological developments enable us to take large strides forward in the construction of new tools. The experience with operating accelerators and storage rings has provided much of the basis for this advance. Impressive performance has been achieved at a number of electron storage rings in several laboratories, and the operation of the 30 GeV proton-proton intersecting storage rings (ISR) at CERN is extremely successful. The start of operation of the 300-400 GeV FERMILAB facility has further demonstrated the advance of the technology.

These accelerators and storage rings have provided excellent opportunities for the detailed study of many aspects of particle beam dynamics. Analysis of the performance of these devices, together with extensive theoretical work, has changed our state of knowledge from a poorly understood "art" to a fully developed science. This increase in understanding has made it possible to design very high current devices which can be expected to meet design specifications.

The development of new radio frequency power tubes and accelerator cavities at SLAC are two of the important advances of the current technology. These are essential to the construction of electron-positron storage rings of high luminosity. Application of the technology of superconductivity to the construction of accelerating cavities gives promise for further advances.

Superconducting materials already have been used very successfully to produce strong magnetic fields in large-volume bubble chambers. One of the recent incisive advances in the field of accelerator development has been the use of superconducting technology for the production of pulsed guide fields. Not only can guide fields at least twice as strong as those provided by conventional means be obtained, but large savings in size and electrical power can be achieved. The great advances in cryogenic and vacuum techniques have been responsible for making available large scale industrial equipment essential to this technology. The required superconducting materials are also becoming available on an industrial scale. Conversely, any major application of superconducting technology to accelerators is likely to stimulate the use of these techniques for a wide variety of applications of general public interest. These include the generation, transport and storage of electrical energy, and use in motors and transportation equipment.

Paralleling all of these efforts is a greatly increased sophistication in the field of control and monitoring. Because of the complexity of accelerator and colliding-beam systems, such progress is most essential. Fortunately, the computer technology and its applications by machine designers have kept pace with this need.

In the area of particle detection for high energy physics experiments, one of the most notable developments has been the very extensive use of computer and integrated circuit techniques for the processing of information obtained from large scale, multiple channel devices. In addition to providing for the efficient and rapid accumulation of data, such equipment makes many experiments possible which otherwise would be ruled out.

The great advances in accelerator technology and particle detection combine to open new regions in high energy physics for exploration.

Section III

Technical Considerations

The undoubted need for new experiments in elementary particle physics at higher energies has been clearly identified and addressed in earlier parts of this report. The tools for proceeding to higher energy accelerators are already at hand.

Two laboratories have proposed new accelerator facilities for funding in FY 1976, namely, the PEP facility proposed by Lawrence Berkeley Laboratory/Stanford Linear Accelerator Center,[1/] and the ISABELLE facility proposed by Brookhaven National Laboratory.[2/] The technical feasibility of both these proposed projects has been examined by the Subpanel. In addition, the design studies leading up to these proposals have been followed closely by several of the Subpanel members during the past few years.

The Subpanel also discussed, in general terms, the program to examine the feasibility of an energy doubling capability at the FERMILAB which would involve the use of superconducting magnets. It also discussed other possible future projects at that Laboratory.

In the following paragraphs some further comments are given on the two facilities (PEP and ISABELLE) for which specific proposals exist.

In the proposed PEP project, colliding beams of 15 GeV electrons and positrons, injected from the SLAC accelerator, are made to interact in six interaction regions (five of which are available for experiments) with an anticipated luminosity of $10^{32} cm^{-2} sec^{-1}$. The SLAC accelerator has the capability, unique in the world, of delivering a high energy, high intensity positron beam for injection at full energy into the PEP device. The design allows possible addition, in the future, of a proton ring which would allow electron-proton collisions in an extremely interesting domain of both energy and momentum transfer. To retain this option does not add appreciably to the cost of the project.

The PEP proposal is the result of an extensive study at SLAC and LBL that led to a detailed design report which contains a thorough analysis of the expected performance and a description of construction plans. The proposed facility is a logical extension of the existing SPEAR program at SLAC and relies largely on proven technology and reasonable extrapolations in the performance estimates. The design as presented appears to the Subpanel to be a sound basis for construction that can start now.

Some uncertainty exists in the PEP design as to the exact amount of rf power required to compensate for higher mode cavity excitation by the beams. This has been considered and would not appear to indicate large uncertainty in either the cost or the performance of the facility.

The design luminosity is higher than previously achieved in electron-positron storage rings, and may not be achieved at initial turn-on. It appears reasonable, however, to expect that it will be reached after an initial running-in period.

The Subpanel feels that funding of the PEP facility in FY 1976 as proposed is entirely reasonable as far as technical considerations are concerned, and that it can be constructed on the time-scale proposed in the LBL/SLAC report.

The experimental techniques needed to carry out the PEP physics program will be based on prior experience at existing positron-electron colliding-beam facilities. We expect that a large fraction of the apparatus required can therefore use existing technology. The principal challenge is expected to lie in the development of large-area detectors at reasonable cost. The estimated expenditure for initial equipment of $20 million appears to be adequate.

The ISABELLE proton colliding-beam machine, as proposed by BNL, constitutes a natural next step in energy and luminosity beyond the 30 GeV ISR machine at CERN. Protons from the existing 30 GeV accelerator (AGS) at BNL would be injected into and stacked in the ISABELLE machine and then accelerated to 200 GeV. Four interaction regions are provided for experiments with anticipated luminosities up to $10^{33}\text{cm}^{-2}\text{sec}^{-1}$. Possible future additions to the facility might include the capability to collide beams of protons with beams of electrons, deuterons or antiprotons.

The ISABELLE proposal is the result of several years of detailed study by BNL of the feasibility of high energy proton-proton colliding beam rings and how they should be constructed. Their conclusions on this have been presented in a detailed design-study report which contains a thorough analysis of the expected performance of the device, and an evaluation of how technically to realize such a project. The Subpanel feels that this establishes a sound basis for proceeding into the construction phase.

The estimated luminosity (which is the main performance parameter) of $10^{33}\text{cm}^{-2}\text{sec}^{-1}$ is substantially higher than has been achieved in the only existing colliding-proton-beam device, the ISR at CERN. In arriving at this figure both the knowledge acquired over the last few years at the ISR, and the recent improved theoretical understanding of beam dynamics, have been taken into account. Further, the figure quoted is also in line with luminosity estimates carried out by other groups for similar projects. A few uncertainties still remain, the main one being the maximum beam-beam interaction that can be tolerated, where further studies, in particular at the ISR, may help to clarify the situation. Another uncertainty is related to the acceleration from 30 GeV up to 200 GeV of beam currents as high as 10 amperes. Finally, some uncertainty also exists as to the method of safely dumping beams with several tens of megajoules of kinetic energy so as to avoid an unacceptable fraction of the beam striking the magnets or other equipment. The Subpanel agrees with the conclusion presented by the study report that these problems can very likely be overcome and that the aim of $10^{33}\text{cm}^{-2}\text{sec}^{-1}$ is a reasonable one, although a considerable running-in period may be required to achieve it.

Another important aspect of the design is the proposed use of superconducting magnets to reduce the size of the rings, to enhance the performance, and to reduce the operating costs. In this, the proposal takes advantage of the important research and development work carried out at BNL and other laboratories in recent years. The Subpanel believes the field of superconductivity is now sufficiently advanced so that the technology can be applied in a project like ISABELLE. It feels, however, that the time allowed to get from the present model stage to a state of being ready to order such magnet systems on an industrial scale has been underestimated, and recommends that the proposed time scale be extended at the front end by one to two years. This will allow time for the large-scale prototype work and will also make it possible to expand the superconductivity group at BNL from a research team to a construction group.

In conclusion, the Subpanel feels that BNL has proven that, from technical considerations, funding of ISABELLE in FY 1976 is reasonable. In that case, a total project time-schedule extended by one to two years at the front end, should be adopted.

For a portion of the experimental program planned for ISABELLE, the apparatus can be similar in concept but not in scale to that used to perform corresponding experiments at the CERN ISR. In general, this class of experiments can be performed using luminosities approximately two decades below the ISABELLE design value. However, a significant portion of the program calls for the maximum available luminosity. Instrumentation of some of these experiments will present major technical challenges; rare events must be measured over a large solid angle in the presence of high background rates. The interest in these particularly difficult but important experiments will certainly stimulate the development of powerful new detection techniques.

It is important that the necessary development of experimental apparatus for PEP and ISABELLE be initiated early enough in the facility construction so that the apparatus will be ready when these facilities come into operation.

References:

[1] A Proposal for a Positron-Electron Colliding-Beam-Storage-Ring Project (PEP) LBL 2688/SLAC 171 (April, 1974).

[2] A Proposal for Construction of a Proton-Proton Storage Accelerator Facility ISABELLE, BNL 18891 (May, 1974).

Section IV

Recommendations and Comments

I. We recommend authorization in FY 1976 of the LBL/SLAC proposal for the construction at SLAC of a 15 GeV electron-positron colliding beam facility ("PEP") with a design luminosity of 10^{32}cm.$^{-2}$sec^{-1}.

II. We recommend providing funds in FY 1976 of $3-4 million for BNL to complete fabrication of prototypes of superconducting magnets, with the aim of making a request for early construction at BNL of a proton-proton colliding beam facility ("ISABELLE") with a design luminosity of 10^{33}cm^{-2}sec^{-1} and a beam energy of at least 200 GeV.

III. We recommend that funds be provided to support an accelerator development program at FERMILAB directed toward the long-term goal of fixed target and/or colliding beam systems in the region of 1000 GeV and above.

Comments on Recommendation I

The Subpanel has found that the PEP project is ready for construction. The experimental program proposed is highly topical and is directed toward central problems of particle physics. The results will elucidate important questions, such as the production of hadrons by electromagnetic forces, the validity of quantum electrodynamics, the interrelation of weak and electromagnetic interactions, and other related questions. The experimental possibilities are wide in scope, and a large number of important experiments will be performed with this device over a period of many years. The cost of construction as estimated in the proposal, is $53 million (FY 1974 dollars). It is expected that the cost of equipping the new installation for the research program will be at the level of $20 million and that the PEP program will add $4 million per year to the cost of operating the SLAC Laboratory. The project represents a collaborative effort of SLAC and LBL; it thus makes use of a group of extremely competent physicists and engineers with extensive experience in designing and constructing high energy facilities, in particular electron-positron storage rings. The availability of the SLAC linear accelerator as an injector for PEP is a unique feature of this project and it will contribute greatly to its performance and reliability.

Construction of PEP will assure continued leadership in the exciting field of positron-electron physics. The importance and scale of the project is such that wide national participation in the research activities is essential and the Subpanel expects the installation to be operated as a truly national facility.

Comments on Recommendation II

A 200 GeV proton colliding beam system represents a seven-fold increase in center-of-mass energy over that of the ISR now operating in Europe. Such a system will enable the United States to take the leadership in this extremely important field of investigation for which no U.S. colliding-beam facility exists today. This project will open a large new energy region for the production of new particles and the discovery of new fundamental phenomena.

Project ISABELLE is the result of several years of detailed study by BNL of the possibilities of high energy proton storage rings. The project takes advantage of the pioneering research and development work on superconducting magnets at BNL and uses the experience derived from the successful operation of the ISR at CERN.

The Subpanel attaches considerable importance to a rapid and effective start of project ISABELLE in order to realize its great potentialities. We therefore recommend substantial funding in FY 1976 to allow the full establishment of magnet fabrication procedures on an industrial scale prior to construction. In addition, we urge increased research and development support in FY 1975. Such commitments of support are normal and necessary parts of preconstruction efforts of a project of this scale and novelty.

It should be noted that the project represents a very large step in the use of superconductive systems, and the lessons to be learned will certainly have a wide impact on many fields of science and technology. Furthermore, research in this new energy region will require the extension of experimental techniques which will lead to new and valuable innovations. The cost of construction of project ISABELLE, as estimated in the proposal, is $127 million (FY 1974 dollars).

The choice of 200 GeV is based on the desire to reach a new important region of energy at a cost within the scope of a realistic program. A higher energy would be more desirable but the 200 GeV level promises to open up many new horizons.

Comments on Recommendation III

A fixed-target proton accelerator offers a much wider range of experimentation than does a proton colliding beam facility at the same center-of-mass energy because it provides higher yields and high energy secondary beams of a variety of particles such as neutrinos, mesons, and hyperons. On the other hand, a colliding beam facility reaches a much higher effective energy. For example, the 200 GeV proton colliding beam system has the same effective energy as an 86,000 GeV (86 TeV) fixed-target machine whose cost would be enormous. It is essential for a balanced program to include colliding-beam systems with the highest possible effective energy, together with fixed-target accelerators of lower effective energies but with much wider potentiality. The former are restricted windows into the world of highest energies; the latter allow thorough exploration of a wide variety of phenomena.

We believe that an accelerator in the few TeV range should be the long-term goal and challenge for FERMILAB. A substantial development program is recommended which contains such features as the energy-doubler project and other steps toward attaining energies in the TeV range and above. The intermediate steps of such a program may well be useful for physics research either with fixed targets or for colliding beams.

The Subpanel foresees the need for a multi-TeV accelerator within the coming 10-15 years. It is hoped that technical innovations will permit the construction of a multi-TeV fixed-target machine with adequate experimental facilities within the budgetary constraints currently visualized. If no technical innovations are found to reduce the cost substantially, an international approach might be advisable, as an expansion of the current cooperation in High Energy Physics, either as a U.S.-European effort or on an even wider scale with the inclusion of the Soviet Union and/or other countries.

General Comments

The Subpanel feels that by FY 1976, eight years after the authorization of the last major construction project, the time is ripe for new steps that will ensure the continued viability and success of the U.S. high energy physics effort. Such action is particularly timely because of the great scientific promises and expectations that have accumulated at the frontier of this science.

The three recommendations represent steps that need to be taken to provide the tools necessary for a comprehensive attack. The Subpanel is well aware of the fiscal constraints that apply to all aspects of our present economy, but feels that its recommendations can be managed within those constraints. The Subpanel believes that the three recommendations can be implemented over the next ten years keeping the Federal budget (AEC plus NSF) for Operations and Equipment near $200 million per year (FY 1974 dollars). Past experience shows that an average expenditure for new and innovative facilities of 20-25 per cent of the total annual amounts for high energy physics provides a reasonable balance between utilization of existing facilities and developments for the future.

The first two recommendations correspond to a construction program which is well within such projections. The projections permit the completion of the construction of PEP and ISABELLE and initiation of research 5-8 years from now at these installations. They will allow the development of the FERMILAB facilities both for the better utilization of the present machine and for progress toward higher energies. They will also allow a number of other necessary additions and highly desirable improvement programs, including electron-proton facilities obtained by adding either an electron beam to the proton-proton project or a proton beam to the positron-electron facility. The Subpanel assumes in these projections that some of the activities at lower energies with the presently existing machines would be reduced considerably or phased out as the new facilities come into operation.

In making the recommendations, the Subpanel took into account the desirability of having the new ventures distributed geographically. The Subpanel as well as the community of physicists feels strongly that such a dispersal (PEP on the West Coast, TeV in the Midwest, ISABELLE on the East Coast) provides the diversity of physics, style, and intellectual input which is desirable for a fruitful scientific endeavor.

Glossary of Technical Terms

AGS	The Alternating Gradient Synchrotron, a 30 GeV fixed-target accelerator located at Brookhaven National Laboratory.
cavity	A radio-frequency resonator, used in high energy physics to accelerate particles.
center-of-mass energy	The energy available in a collision to make new particles--the effective energy of a collision.
CERN	The European Organization for Nuclear Research, located in Geneva, Switzerland.
colliding beams	A means of attaining very high energy collisions by accelerating two beams of particles and colliding them head on.
cross section	The effective area of a particle in producing a certain type of reaction.
cryogenics	The technology of very low temperatures.
electromagnetic interaction	The electric and magnetic forces that all charged particles experience; transmitted by the photon.
e^+e^-	positron-electron.
ep	electron-proton
ev	An electron volt; a unit of energy equal to the energy an electron gains when accelerated across a one volt potential gap.
fixed target accelerator	A machine which accelerates a beam of particles and collides it with stationary matter.

GeV	10^9 electron volts (Gigavolt), a unit of energy.
hadron	A particle with strong interactions; e.g., the proton.
hyperon	A heavy particle which decays via the weak interaction into protons and pions.
inelastic scattering	A collision in which the resulting particles are not identical with the initiating ones.
ISR	The Intersecting Storage Rings at CERN, the only proton-proton colliding beam facility in the world.
lepton	A particle with weak and without strong interactions. The known leptons are the electrons, muons and neutrinos.
luminosity	The rate at which particles collide in an accelerator or colliding beam.
meson	A strongly interacting particle (hadron) which is emitted and absorbed by nucleons and other hadrons. The mesons probably transmit the strong force, in the same way that the photon transmits the electromagnetic force.
neutrino	A massless particle emitted with the speed of light in some radioactive processes. It interacts with other particles only via the weak interaction.
nucleons	protons and neutrons.
photon	A particle of light. The photon is also the transmitter of the electromagnetic force.
pp	proton-proton

positron	An anti-electron, i.e., an electron which has positive charge. A positron and an electron annihilate into photons.
quark	A hypothetical particle which may be the basic constituent of all hadrons.
rf	Radio frequency; refers to the electromagnetic field used to accelerate particles.
SPEAR	The e^+e^- colliding beam facility located at the Stanford Linear Accelerator Center.
storage ring	A circular evacuated ring in which beams of particles are stored in order to make head-on collisions, i.e., a colliding-beam facility.
strong interactions	The strong forces between hadrons which hold the nucleus together; probably transmitted by the mesons.
superconductivity	The phenomenon that certain metals at very low temperatures become perfect conductors, allowing accelerator components with very high currents and very low power.
TeV	10^{12} electron volts (Teravolt), a unit of energy.
threshold effect	The onset of production of a new particle when the effective energy available becomes larger than its mass.
W particle	A hypothetical heavy particle, which is thought to be the carrier of the weak interaction.
weak interaction	The force responsible for one type of radioactive decay; possibly transmitted by the hypothetical W particle.